THE

AMERICAN JOURNAL

OF THE MEDICAL SCIENCES

FOR APRIL 1872.

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All articles intended for the *Original Department* of this Journal must be contributed to it *exclusively*. The insertion elsewhere of *abstracts* of papers *prior* to the publication of the entire paper in this Journal is a violation of this rule. As original articles are *accepted only on this condition*, we consider those who favour us with contributions to be bound in honour to conform to it.

Contributors who wish their articles to appear in the next number, are requested to forward them before the 1st of May.

Compensation is allowed for original articles and reviews, except when illustrations or extra copies are desired. A *limited* number of extra copies will be furnished to authors, *provided the request for them be made at the time the communication is sent* to the Editors.

The following works have been received:—

St. Andrew's Medical Graduates' Association Transactions, 1870. London: J. & A. Churchill, 1871.

St. Thomas's Hospital Reports. New Series, Vol. II. London: J. & A. Churchill, 1871.

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A Clinical Manual of the Diseases of the Ear. By Laurence Turnbull, M.D., Physician to the Department of Diseases of the Eye and Ear of Howard Hospital of Philadelphia, etc. With a coloured lithographic plate and over one hundred illustrations on wood. Philadelphia: J. B. Lippincott & Co., 1872.

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Emergencies and How to Treat Them: the Etiology, Pathology, and Treatment of the Accidents, Diseases, and Cases of Poisoning, which demand Prompt Action. Designed for Students and Practitioners of Medicine. By JOSEPH W. HOWE, M.D., Visiting Surgeon to Charity Hospital, etc. New York: D. Appleton & Co., 1871.

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The American Journal of Science and Arts. January, February, March, 1872.
The American Naturalist. January, February, March, 1872.
The Boston Journal of Chemistry. January, March, 1872.

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THE

AMERICAN JOURNAL

OF THE MEDICAL SCIENCES

FOR APRIL 1872.

ART. I.—*Remarks on the Pathological Anatomy, Causes, and Treatment of Rickets.* By JOHN S. PARRY, M.D., one of the Attending Accoucheurs to the Philadelphia Hospital.

IN the January number of this Journal, we pointed out the frequency and symptoms of rachitis. As very diverse views are still promulgated in regard to the subjects to be discussed in this article, it was thought that some remarks upon them might not prove uninteresting. Various opinions have been expressed in regard to the nature of the bone alterations. Virchow,[1] Gerdy,[2] and Vogel[3] deny that there is any true malacia. Willshire,[4] Jenner,[5] Smith,[6] and Niemeyer[7] all assert that there is not only deficient ossification of the growing bones, but that in addition to this there is softening and resorption of the calcareous salts, which have already been deposited.

The decision of this question is of primary importance, for, so long as the point remains unsettled, an effectual bar is reared to our further progress towards the truth.

If Jenner, Smith, and Niemeyer are correct, we cannot see wherein Beylard,[8] Trousseau,[9] and others have erred in believing mollities ossium and rachitis to be one disease.

The distinction between these depends upon this point, and while we believe that the two affections are associated as regards their nature, we

[1] Virchow's Archives, Band 5, heft 4; and Edinburgh Med. Jour., April, 1854.
[2] Brit. and For. Med.-Chir. Review, July, 1856, p. 67.
[3] Dis. of Children, New York, 1870, p. 521.
[4] Brit. and For. Med.-Chir. Review, July, 1856.
[5] Med. Times and Gaz., May 12th, 1860.
[6] Wasting Diseases of Children, Phila., 1870, p. 89.
[7] Prac. of Med., N. Y., 1869, p. 507.
[8] Du Rachitis, etc., Paris, 1852.
[9] Clinique Medicale, vol. iii., Paris, 1865.

cannot but think that they are essentially different, and while the distinguishing feature of mollities ossium is the resorption of the calcareous salts from the fully formed bone and consequent softening of it, we as emphatically believe that in rickets the lesion consists simply in deficiency of ossification, the already formed bone remaining unaffected.

This opinion is supported by the pathological characters of the disease, and the natural history of the development of bone. The union of the animal and earthy substances of this tissue is so peculiar that we have every reason for believing that either one of these morbid processes may go on alone, for the earthy salts, as we well know, may be separated from the animal basis without destroying the shape of the skeleton, proving that there is something in the connection between them which cannot be explained by ordinary chemical laws. There seems to be no evidence that the animal portions of the bones are involved in the rachitic process, though it must be remembered that Jenner[1] seems to think that they are, and that Lehmann and Marchand[2] have asserted that there is an alteration in the chemical composition of these structures. This observation, however, has not been confirmed by other investigators.

The appearances presented by a rickety bone vary according to whether it is long or flat. As the alterations in the former are the most obvious during life, they may be advantageously studied first, and in doing so it is to be remembered that we are dealing with a growing and developing bone, and that, aside from the curvature of it, about which nothing will be said, its growth may be involved in two ways: first, through the cartilages, by which it is increased in length; and secondly, through the periosteum, by which it is augmented in thickness. The most striking external character of one of these bones is the epiphysial enlargement, the degree of which varies in different cases. If the disease is well developed, the contiguous surfaces of the diaphysis and the articular extremity of the bone appear to be approximated, as though the tissue between these points had been compressed by two opposing forces. In some instances, the structures along the epiphysial line give way, and the part looks as if the soft tissue had been squeezed out. This is an important alteration of the disease, and affords an explanation for some very characteristic changes in the adult rachitic skeleton. In some of these, the epiphysial lines are peculiarly knotty and irregular, the bulging extending partially or wholly around the bones, as Mr. Humphry[3] says, looking "precisely as if the bones, when in a soft state, had been squeezed out at these parts by pressure made upon the two ends." This material may become calcified in this abnormal position, and thus interfere still farther with the growth of the bone. Bulging at these epiphysial lines, which is not by any means a constant pathological character, but rather an accident of the disease, is to

[1] Medical Times and Gazette, 1860, vol. i. p. 621. [2] Ibid.
[3] Trans. Medico-Chir. Society, London, 2d series, vol. xxiii. p. 314, 1862.

be distinguished from the enlargement of the extremities of the bones, which is a uniform increase in size, which occurs only in infancy, and which has always disappeared in the skeletons of adults, who have been rachitic when young. On the contrary, bulging of the epiphysial lines is usually, if not always, permanent. It is most frequently noticed at the distal extremities of the femur and radius, and the proximal extremity of the humerus, and I have no doubt that it is produced as Mr. Humphry suggests,[1] by pressure upon the opposite extremities of the bone. The study of these changes of the epiphysial lines is not altogether without interest, as the alteration affords an explanation for some of those cases in which the rachitic shortening of the extremities is greater upon one side than it is upon the other. It may serve also to explain other deformities of the disease, as certain cases of knock-knee, in which the epiphysial line has given way upon one side and not upon the other.

If the periosteum be stripped from one of these long bones, as well as the flat ones, it is apt to appear slightly thickened and more vascular than in health, though in uncomplicated cases it never presents any evidence of inflammation, though Meyer has asserted that it does.[2] After the membrane is stripped off, small portions of the newly-formed and morbid bone are found adhering to it. Immediately beneath the periosteum, this recently-formed bone presents a peculiar appearance. It is soft, spongy, and succulent, filled with a soft "pumice-stone" material, which is of a purple-red colour. The colour is developed just in proportion to the severity of the disease.[3] The tissue thus formed is very easily cut or scraped away with a knife, and resembles the diplöic structure of the bones of the skull.

These changes are better marked in the flat bones, and especially in the frontal, occipital, and parietal, than they are in the shafts of the tubular bones, but it is important to remember that they are not by any means peculiar to the former, though much more prominence has been given to them in connection with the cranial changes, than with those which occur in the diaphyses of the long bones. While the rachitic process tends to attack certain parts more than it does others, or at least while it makes itself more apparent in some parts than it does in others, it affects ossification wherever it is going on, whether from membrane or from cartilage.

Viewed as a whole, the shafts of the long bones are diminished in circumference. Writers are not by any means agreed in regard to the truth of this statement, as some assert that they are thicker than normal. Willshire,[4] Rindfleisch,[5] and Ormerod,[6] however, take the opposite side of the question, an opinion with which the writer agrees in the main. In

[1] Loc. cit.

[2] Henle and Pfeufer's Journal, vols. iii., vi., new series, quoted by Willshire.

[3] Vogel, Diseases of Children.

[4] Op. cit.

[5] Text-Book Path. Histology, Phila., 1872.

[6] St. Bartholomew's Hosp. Reports, vol. vii.

our previous article,[1] attention was called to the fact that the growth of the whole skeleton is interfered with in this disease, and that in well-marked cases all of the bones are short and poorly developed. Not only are they short, but, with few exceptions, they are likewise thin. In some instances, owing to the rapid formation and imperfect ossification of the large quantity of the spongy new tissue, they appear to be thickened, though the whole amount of osseous substance is below rather than above the normal standard. In consequence of the accumulation of this soft porous osseous tissue, and the deposit of lime-salts in it during the progress of recovery, the bone sometimes becomes much thicker and heavier than is normal, though this is not one of the essential characters of the disease. Excepting the occipital, the bones of the cranium are nearly always thickened in rachitis, and after recovery they are often found greatly hypertrophied. Sir Charles Bell speaks[2] of "rachitic skulls which were seven-eighths of an inch in thickness," and mentions the importance of remembering this fact during the operation of trephining an individual who has suffered from this disease during infancy. The writer has seen skulls almost as thick as those described by Bell, and has repeatedly examined the bodies of children four or five years of age, the thickness of whose skulls exceeded that of a healthy adult cranium.

The cause of the difference in the thickness of the long and flat bones in this disease is the difference in the rate at which the absorption of old bone is going on. In some of the flat bones, as in the scapula, this is so modified as to form no counterpart to the same process in the tubular bones. In the cranium the resorption which is constantly going on from the interior of the skull is so modified, and is intended to effect such different ends, that it influences the progress of the disease which we are discussing. Herein lie the apparent differences of changes which are identical in nature, and had these facts been remembered, various discussions in regard to the nature of cranio-tabes, and other cranial alterations of the disease, would not have occurred. It is to be remembered that it is the calcification of the new bone which is arrested in rachitis, while the periosteum continues to prepare the materials for the reception of the calcareous salts, just as in health. At the same time that this is going on, the resorption from the interior of the long bones, and the formation and increase in size of the medullary cavity, are proceeding normally. For this reason the tubular bones become light and fragile, and their true osseous tissue is diminished and not increased. If a section of one of these bones from a rachitic child, who has reached the latter stages of the disease, be made, the compact tissue will be found near the centre of the bone, while on the exterior is a mass of soft, spongy material.

The appearances presented by a longitudinal section of one of the long

[1] Amer. Journ. of Med. Sciences, Jan. 1872. [2] Lancet, April, 1841.

bones are very important. It is not our purpose to describe them very fully. This has been done by Virchow,[1] Rindfleisch,[2] and others. The most noticeable alterations, and those which are characteristic of the disease, are to be found along the lines of ossification at the epiphysial extremities of the bones. In healthy persons, this process (ossification) progresses along the extremity of the diaphysis in a perfectly straight line, which is clear and sharply defined, though so narrow as to be little more than visible to the unaided eye. In rachitic bones this line of ossification instead of being straight is very irregular, and the "proliferating zone" of the cartilage is enormously thickened, so that it will measure several lines in width. Moreover, the medullary spaces may project beyond the line of ossification into the layer of proliferating cartilage.

Now, this change is not equally well developed at the epiphysial extremities of all bones, nor is it probable that it indiscriminately manifests itself at either the distal or proximal extremity of a bone. The word "probable" is intentionally used in this place, for the opinion which we are about to express is the result of recent observations, and, though rickets is a frequent disease, the writer may not be able, for some time, to obtain further post-mortem evidence. Jenner[3] has asserted that rachitic epiphysial alterations are as apt to involve the deep-seated as the superficial joints. The writer's own belief is, that the extremity of the bone *from which* the nutritious artery is directed is the most severely, the most frequently, and the earliest involved. We would therefore find the upper extremity of the humerus more frequently affected than the lower, while precisely the opposite is the case with the femur, the distal end of this bone being the one which is most frequently involved. In the humerus the nutritious artery is directed downward, while in the femur and radius it runs upward. In the tibia it runs downwards, and the proximal end of this bone is the one which is usually involved first and most severely.

It is doubtful, however, whether this law, if it be found to apply generally to the disease, is the result of the manner of the distribution of the blood and a difference in the amount of that fluid received by the two extremities of the bone. Because the nutritive artery takes a certain direction, it does not follow that the extremity from which it is inclined receives less blood than its fellow. From the experiments of Mr. Humphry,[4] the direction of this vessel is determined by the unequal growth of the two extremities of the bone. Whatever may be its course as regards the bone, it always slants in the same direction in the same bone, and always runs towards that end at which the epiphysis is first united to the shaft. The law which governs the union of the epiphysis does not seem to be one of nutritious

[1] Cellular Pathology, Transl. by Chance. N. Y. p. 477.
[2] Op. citat., p. 565.
[3] Medical Times and Gazette, 1860.
[4] Trans. Medico-Chirurg. Soc. of London, vol. xliv. p. 124.

supply, but rather one of the mechanical wants of the skeleton. Mention has already been made of bulging of the epiphysial lines in the skeleton in this disease. Now, these are the weakest portions of the bones, and if they were situated in the middle of these they would not, for one moment, bear the strain of the superincumbent body or even that of muscular action. The consequence is, that these lines of ossification are located at the extremities of the bones, and they, with the cartilaginous portions of these, are soft and vascular, just in proportion to the rate of their growth. Hence, we find that ossification goes on most rapidly at that end of the bone, the strength of which is the least likely to become impaired by the attendant changes, and this is in all cases the broader and the larger of the two. In the humerus the disadvantages of situation and the marvellous delicacy of manipulation attained by the forearm and hand, require that the elbow should be perfected comparatively early. Consequently, we find that the proximal end of the bone grows more rapidly than the distal, and that the nutritious artery runs towards the latter. The upper extremity of the femur, as it terminates in a rounded extremity, which at an early age is placed nearly at right angles with the shaft of the bone, is taken at great disadvantage, and consequently will not bear additional weakening by rapidly advancing growth and ossification. The distal end, however, is broad and flat, receives the pressure which it has to bear directly and in the most advantageous manner; hence, at this end, growth advances far more rapidly than it does at the other. The same is true of the tibia. Its malleolar extremity is taken at a disadvantage; the upper is broader and stronger, and ossification advances most rapidly here, while the nutritious artery runs towards the distal end of the bone. These facts being remembered, it is easily seen why rachitic epiphysial enlargement manifests a preference for that extremity of the bone, away from which this vessel is inclined. It is simply because growth is going on at that point with the greatest rapidity, the natural consequence of which is that any interference with the process of ossification would appear more prominent at that than at the opposite extremity.[1]

Hence, we see that nature, ever careful of her resources, and ever cherishing her work with the utmost tenderness, so guides the growth and development of the skeleton, that it is evolved in accordance with such mechanical laws as are best adapted for its protection. And so the disease, which we are discussing, we find first affects those parts which are most

[1] "Morbid action is most frequent in that part of the bone within which the area of development is the largest and most active. Thus, we see necrosis of the femur of the adolescent almost invariably occurring at the lower portion of the femur, since it is here that the greatest degree of vascularity is found. In the humerus its proximal end is under similar circumstances the one prone to disease."—Dr. Harrison Allen, "Localization of Diseased Action in the Osseous System," *Amer. Journ. Med. Sci.*, Oct. 1870, p. 407.

protected and where it will do the least damage. The extremity of the bone from which the nutritious artery is directed is softer than its fellow in the well-developed disease. It can often be compressed between the fingers or bent by the slightest force.

In healthy bones the length increases much more rapidly at the cartilaginous extremities than the thickness of the shaft does beneath the periosteum. The change in the former is, therefore, the most important alteration of the process of ossification, and its effects upon the economy of its victim are very distressing, and are unlike those produced by any other disease whatever; but, while the arrest of ossification at the epiphyses is the most important in its effects on the skeleton, the periosteal alterations are the source of immediate difficulty. In most cases the deformities which occur during the active stages of the disease are the consequences of the arrest of subperiosteal ossification, and it is only in more rare instances that distortion results from giving way in the epiphysial lines, as has been described. Another important consequence of these periosteal lesions is fractures of the bones. According to Guersant,[1] one-third of eighty cases of fractures under his care occurred in rachitic children. In only rare instances is the periosteum torn in these. In others only one side of the bone is broken, the "infraction" of some writers, and the "green-stick fractures" of others. In both instances the medullary cavity is destroyed and the medulla wounded and broken, a condition which Jacobi[2] suggests may be very important in its bearings upon the health of the child; since it is asserted[3] that the medulla performs an important part in elaborating the blood-cells. If this opinion is proved to be correct, certain cases of rachitis will present remarkable facilities for the study of this subject, since these children are not unfrequently the victims of multiple fractures.

The minute alterations in the bones in rachitis are extremely interesting, and have been made the subject of study by Kölliker[4] and Virchow.[5] Both of these investigators used them to study the process of normal ossification, a subject about which there has been a vast amount of discussion, and in regard to which our knowledge is still unsettled. According to Virchow,[6] the essential lesion of rachitis is a deficiency of calcareous salts. He believes that everything goes on normally until these should be deposited, and that the preparatory processes are naturally carried on. Jenner[7] holds that there is not only a deficiency in the bone salts, but that there is also an error in their deposition, that is, they are thrown down in abnormal places, the osseous matter being formed in the cartilage cell as well as in the matrix. This observation, as well as his statement

[1] London Lancet, 1846, vol. i. p. 148. [2] Amer. Journ. of Obstetrics, Nov. 1870.
[3] Neumann, Phila., Med. Times, vol. i. p. 200.
[4] Microscopical Anatomy, Philada., 1854.
[5] Cellular Pathology, Chance's Translation.
[6] Edinburgh Med. Journ., April, 1854. [7] Med. Times and Gazette, 1860.

that the salts previously deposited are reabsorbed, has not been corroborated by other observers, so far as we are aware.

In short, therefore, it seems that the preparatory processes of bone formation, whether submembranous or cartilaginous, go on perfectly, until just at the time when solidification should commence, when these are arrested. Humphry,[1] Kölliker,[2] Vogel,[3] and Rindfleisch,[4] all agree in this opinion. According to Ormerod,[5] the bone corpuscles undergo certain remarkable changes in this disease. This author has found them to be peculiarly elongated as if two, or three, or even more had come in contact and fused at their extremities. The canaliculi from these are greatly increased in number. The origin of this condition of the bone corpuscles is a matter of some doubt.

The cause of this imperfect ossification in rachitis is a matter of interest. The absence of the carbonate and phosphate of lime being the principal characteristic of the bones in this disease, it has been asserted by some that these salts pass off from the body through the kidneys and other channels. Lehmann says[6] that the urine of a rachitic child contains four times as much phosphates as it does in health. Marchand,[7] Benke,[8] and Aitken[9] all found an excess of phosphates in this fluid, while Friedleben[10] and Hillier[11] were unable to discover any increase in their quantity. The collection of the urine of a child as young as those who are affected with this disease is so difficult that the writer has been unable to procure a sufficient number of specimens to make his assertions on this point altogether reliable, but as far as his observations have yet gone, they confirm those of Friedleben and Hillier. He is at least convinced that cases of the disease occur in which the urine presents no peculiar or characteristic changes, and there seems to be no reliable evidence that these salts leave the body by this channel instead of being deposited in the bones.

Guersant[12] has called attention to the fact that a diarrhœa will delay the union of a fracture in a rachitic subject. The same he asserts is true of other diseases,as acute bronchitis and pneumonia, and Friedleben,[13] as late as 1860, asserted that bronchitis was one of the causes of the disease. But those who may think that these discharges may form an outlet for these salts

[1] Medico-Chirurg. Trans., vol. ix. p. 312.
[2] Op. citat.
[3] Op. citat., p. 521.
[4] Op. citat.
[5] St. Barthol. Hosp. Rep., 1871.
[6] Schmidt's Jahrbuch, 1843, Bd. 39. S. 8. Quoted by Hillier.
[7] Jour. fur Pract. Chemie, 1842. Quoted by Hillier.
[8] Zur Phys. und Path. des phosph. und oxal. Kölkes: Göttingen, 1850. Quoted by Hillier.
[9] Reynolds' Syst. of Med. vol., i. p. 775.
[10] Hillier, Diseases of Children, Phila., 1868, p. 101.
[11] Ibid., p. 101.
[12] Phila. Med. Exam., April, 1846.
[13] New SydenLam Soc. Year Book of Med. and Surg., 1861, p. 399.

need only be reminded of the fact that constipation[1] very frequently attends the disease during a large part of its early stage, and that it is exceedingly frequent as a precursor of the first bone changes. Nor can inflammation of the bronchial mucous membrane and its consequent discharge be looked upon as the cause of the non-deposition of the earthy constituents of the bone, for, while this is one of the most frequent and constant complications of the disease, we have seen many cases in which it did not appear until some of the unequivocal bone lesions of the affection had set in. Nor do we know that there is any evidence that the blood is deficient in the phosphates and carbonates of lime in rachitis. The results of the oft-repeated attempts to cure the disease by the administration of these salts have proved the truth of this opinion. Because the osseous changes are among the most common, the most constant, and by all means the most important of the anatomical alterations of the disease, rachitis must not be looked upon as an affection of the osseous tissue alone, for, as it has been said, there are evidences of want of growth and development in almost all other parts of the body. This deficiency in the deposit of the earthy salts in the bones can no more be looked upon as the essence of the disease than can ulceration of Peyer's patches that of typhoid fever.[2] It, like the latter, is a constitutional affection, and, as the lesion of the intestinal glands is the chief alteration in enteric fever, so the arrest of calcification is the most striking and the most important pathological change in rachitis.

The immediate cause of this arrest in the deposit of the lime salts is to be determined by observation of the circumstances under which normal calcification occurs, and a study of the laws which govern the process. The largest portion of the phosphates of lime in the human blood is in the serum, in which it exists as a tribasic phosphate. In the bones themselves there is a combination of the bibasic and tribasic phosphates. These salts are insoluble in water or alkaline fluids, but are freely soluble in any liquid which contains a free acid. As the blood contains free carbonic acid, Rindfleisch[3] suggests that this may be the agent which keeps the salts of lime in solution. Any explanation of normal calcification is based upon some supposition or an uncertain observation, but the view of Rindfleisch is exceedingly ingenious and very plausible. According to him the deposition of lime is the result of the abstraction of the carbonic acid which holds it in solution. The circumstances under which normal calcification occurs are somewhat peculiar. Genuine osseous substance may be produced in a tissue which is well supplied with bloodvessels, but the lime salts are not deposited indiscriminately in any part of the territory supplied by one

[1] Jacobi, Amer. Journal of Obstetrics, Nov. 1870; and the Author, Amer. Jour. Med. Sciences, Jan. 1872.

[2] Jenner, Op. citat.

[3] Pathological Histology, Phila., 1872, p. 56.

of the capillaries, but, on the contrary, they are thrown down at the points which are furthest from the vascular supply; in other words, where the circulation is the slowest, and where the solvent has an easy opportunity to free itself from the lime salts, leaving them lodged in the tissues on the margins of the nutritive territories. In rachitis peculiar opportunities present themselves for the study of the process of calcification. During the progress of the disease the new medullary spaces extend far beyond their normal limits, projecting like granulations on ulcerating surfaces at various points along the line of ossification. If a section of the extremity of the bone be made in its transverse diameter immediately through, or rather just above, the line of ossification, and this be examined under a low power, the lime salts will be found at the points which are most distant from the medullary spaces. In other words, they fall down at the margins of the area, which draws its supply from the capillary vessel which ramifies in this medullary space.

Normal calcification occurs in the same manner, and the lime salts are first deposited in those portions of the tissues in which the circulation is exceedingly slow, and which are on the margins of the nutritive districts.

In disease calcification occurs under precisely the same circumstances, and the lime salts are only deposited in abnormal situations when the circulation is arrested or goes on very slowly. Hence, we find it occurring in certain parts, as the larynx, costal cartilages, and other tissues, after a prolonged inflammation.

The application of these principles to rachitis has already been made by Rindfleisch[1] and Jacobi.[2] In the normal bone the circulation is exceedingly slow and the vessels but very sparsely distributed, so that everything favours the deposition of the calcareous salts. The distinguishing feature of rachitic bones, however, is the augmented vascular supply. The periosteum covering the shaft, as already stated, is thicker and more vascular than it is in health, while in no other disease do the medullary vessels extend so far beyond the ossifying line. The section of the bone, too, shows the large excess of blood which it contains. In these bones the circulation is so rapid that the carbonic acid and other agents which hold the phosphate and carbonate of lime in solution have not time to diffuse away from these salts, and hence they are carried past the depots in which they should have been lodged.

If this is true, it is plain that the most important lesion of rickets does not depend upon a deficiency of the lime-salts in the blood, nor does it necessarily depend upon the presence of an excess of their solvents in that fluid. Though the explanation just given may be ingenious, it is not yet satisfactory, for the discovery of the cause of deranged circulation in rachitic bones presents an almost equally difficult problem for solution.

[1] Text Book of Pathological Histology. [2] Amer. Jour. of Obstetrics, Nov. 1870.

Whether this is to be discovered by the study of some correlated affections, is a question which is as yet undecided. Mollities ossium and rachitis have long been associated, and so closely, indeed, do they resemble one another, that many have considered them one disease. The writer has already expressed it as his opinion that they are different, an opinion which is supported by the clinical history and the anatomical lesions of the two affections. In the latter the external characters of the two are strikingly alike; in both the bones are exceedingly soft, so that in extreme cases they can be readily bent or easily cut with a knife. In both the vascular supply is greatly increased, and in both there is great deficiency of the earthy constituents of the bones. Aside from the fact that the two disorders affect persons of different ages and in totally different constitutional conditions, other distinctions are to be found in the minute anatomy of the affections. The essential lesion of rachitis is the deficiency in the deposition of the lime-salts without any change in those which have already been thrown down, while in mollities ossium these are softened and reabsorbed, and, according to Rindfleisch,[1] the first evidence of this alteration in the bone is to be found around the margins of the Haversian canals, while those portions of this tissue which are farthest from these and the medullary spaces remain normal for a longer time. Along the margin of these, the disease progresses, the line of softening being irregular. Indeed, it looks as if the blood circulating in the bone contained some agent, which was capable of effecting a chemical solution of its earthy salts.

Now, though the rachitic and the osteo-malacic processes are not the same, the latter may throw some light upon the production of the former. There is one fact which more than anything else strikes us in the study of mollities ossium, and that is its association with nervous symptoms. Insane persons are peculiarly liable to it, a fact which the frequency of fractured ribs among them fully attests.

Mr. Durham[2] and Dr. Pedler[3] have directed the attention of the profession to this association, and suggested that there is yet some unappreciated lesion of the nervous system which is the source of the disease.

The theory that rachitis is the result of some morbid condition of the nervous system, is almost as old as the history of the disease, for Mayow[4]

[1] Op. cit., p. 594-5. Ormerod (St. Bartholomew's Reports, vol. vii., 1871, p. 41), in the main, corroborates this statement from independent observations. Speaking of a fragile rib from an insane patient, he writes: "The Haversian canals are seen to be large, and the transparency of the innermost concentric rings of the Haversian system becomes a very prominent feature. If we should happen to be using a section stained with magenta, we shall observe that these transparent rings take the dye more readily than the rest, an indication, I believe, of the removal of the earthy matter with which this dye has little affinity."

[2] Guy's Hosp. Reports, 1864.

[3] West Riding Lunatic Asylum Med. Reports, London, 1871, vol. i. p. 172.

[4] Tractatus duo, alter de Respiratione, alter de Rachitide, 8vo., Leyden, 1671.

asserted that it was caused by some "obstruction" in the spinal cord, an opinion which Allen[1] and Hoffman[2] reiterated. It is neither in the brain nor spinal cord that we are to look for the changes which are to explain the production of the essential lesions of the disease, but may there not be some morbid condition of the vaso-motor system of nerves? May it not be that paralysis of these in the young, growing, and developing bone may be the cause of the trouble? In the present state of our knowledge, no one can afford to hazard a positive opinion in regard to this, but the view seems to the writer worthy of some investigation. The manifest vascularity of the affected bones and the periosteum covering them points in this direction. This congestion is not active, at least is not inflammatory, as was suggested by Meyer[3] in regard to the periosteum.

To more fully discuss the etiology of the disease, it may not be amiss to allude to one of the old theories of its causation. The deformities which result from the bending of the bones could not but lead the early writers upon this subject to contrast the rachitic process with the chemical removal of the earthy constituents of the bones, which follows their maceration in dilute acids. It is not strange that Fourcroy suggested that in this disease a substance was formed in the tissues or fluids of the body, which dissolved the carbonate and phosphate of lime just as hydrochloric acid does outside of the body. This theory has at one time or other had many advocates, and, in studying the history of the disease, it is exceedingly curious to see how many times it has reappeared, and how many forms it has assumed. We would not allude to it here, had it not been suggested, in a recent discussion at the Pathological Society[4] of Philadelphia, that cranio-tabes, one of the manifestations of the disease, is due to the accumulation of carbonic acid in the blood which dissolves the calcareous salts in the occipital bone. This excess of the acid was supposed to result from the interference with the oxygenation of the blood which results from the bronchitis with which these children suffer. Such an opinion as this cannot be based upon clinical experience, since any one who has had favourable opportunities for the study of rachitis must have noticed that the minor forms of the disease—cranio-tabes included—are often unassociated with any respiratory difficulty whatever, while in those conditions in which the accumulation of carbonic acid is much greater than is usual in this disease, for example in cyanosis, we have no manifestation of deficiency of earthy salts in the bones. If this chemical theory of the disorder were correct, the bones should be found acid in reaction, but, according to Hillier,[5] they are alkaline, a statement which the writer is able to confirm.

[1] Practice of Physic, London, 1741.
[2] Practice of Medicine, London, 1783.
[3] Op. citat.
[4] Amer. Journ. of Med. Sciences, April, 1871, p. 428.
[5] Diseases of Children, 8vo., Phila., 1868, p. 101.

In the cases in which I have tested them, I have always found them neutral or faintly alkaline.

Some observers have taught that there is a connection between syphilis and rickets. This doctrine is as old as the history of the disease, for Glisson[1] and Van Swieten[2] both combat it. Boerhaave[3] and Astruc[4] supported it in their day, while in later times, Vogel[5] and Hillier[6] both say that syphilis in the parents predisposes the child to this disease. The writer has treated many cases of inherited syphilis in the Philadelphia Hospital, and he has never yet seen it associated with severe rickets. Indeed, he does not feel sure that he has ever met with these two diseases in the same person, though he has repeatedly examined syphilitic infants for rachitis. It seems as if one cachexia excluded the other, and while we do not say that a syphilitic child may not become rachitic, we do say that it is very unlikely to do so.

The relations of these three diatheses, the scrofulous, the tubercular, and the syphilitic, to rachitis, are exceedingly interesting. At various times, the last has been supposed to be nothing more than a form of one of the others, and even until a very recent period, scrofula and rickets have been supposed to be identical. Sir Charles Bell,[7] Simon,[8] Dupuytren,[9] Watson,[10] Whitehead,[11] and Condie[12] all advocated this opinion, while M. Hervieux[13] taught that this disease was nothing more than one of the important symptoms of tuberculosis in children under three years old. These opinions, though they have been promulgated by such excellent authorities, cannot be supported by bedside experience, and, instead of believing rickets to be but a form of scrofula or tuberculosis, I was, soon after commencing the study of the disease, led to conclude that as with syphilis there is an antagonism between them. In certain cases, rachitis may coexist with either of them, but the association is a rare one, and the existence of rachitis excludes the presence of at least the higher manifestations of either of the other diatheses. Of the two, scrofula and rachitis are more frequently met with in the same person than tubercle and rickets.

[1] Treatise of the Rickets, London, 1668.
[2] Commentaries, etc., Edinburgh, 1776.
[3] Aphorisms, London, 1742.
[4] Diseases of Children, London, 1746.
[5] Diseases of Children, New York, 1870.
[6] Diseases of Children, Phila., 1868, p. 94.
[7] Lancet, March 15, 1834.
[8] Animal Chemistry, Syd. Soc. ed., 8vo., London, 1846, vol. ii. p. 284.
[9] Diseases and Injuries of Bones, Syd. Soc. ed., 8vo., London, 1848, p. 28.
[10] Prac. of Med., 4th ed., 8vo., London, 1857, vol. i. p. 206.
[11] Hereditary Diseases, 8vo., London, 1857.
[12] Diseases of Children, 8vo., Phila., 1868, p. 671.
[13] Rilliet and Barthez, Dis. Enfants, 8vo., Paris, tome iii. p. 364.

In only a very few cases have we seen these associated. Indeed, it is very remarkable how these children recover from grave pulmonary complications, and we have frequently seen cases which seemed to present every condition favourable for the deposition of tubercles, but they nearly always recovered perfectly, or else died of some acute complication.[1] It must be concluded that tuberculosis cannot be considered as one of the causes of rickets. Rokitansky[2] and Trousseau[3] say that the two diseases are mutually exclusive. There is certainly such a relation, but the law is not absolute. Willshire[4] has recorded two cases of the disease which were associated with tubercular meningitis, and I have met with three such at the Philadelphia Hospital.

Ritter Von Rittershain[5] asserts that chronic tuberculosis of the father predisposes a child to rickets. Jenner[6] denies this, and our own observations would lead us to believe that he is entirely correct. It seems as if the health of the father had little influence in the production of this disease.[7] Nor is it probable that tuberculosis in the mother predisposes her

[1] Great care should be exercised in the examination of rachitic children, or physical signs will be misinterpreted. In those cases which present considerable thoracic deformity, it is not uncommon to meet with high pitched, intense bronchial respiration, and if the auscultatory signs alone were relied upon, pneumonia or pneumonic phthisis would be diagnosticated. In most cases, percussion will enable the physician to arrive at a correct conclusion, as the resonance remains unimpaired. In other cases, however, slight collapse of the lung produces some dulness, which makes it much more difficult to arrive at the truth, which can only be done by a careful comparison of the phenomena of percussion and auscultation, when it will be found that the intensity of the respiratory physical signs is out of proportion to the diminution of the resonance. Merei (*Disorders of Infantile Development and Rickets*, London, 1855, p. 198) recognized the condition to which attention is here directed.

[2] Pathological Anatomy, Phila., 1855, vol. i. p. 238, and vol. iii. p. 140.

[3] Brit. and For. Medico-Chir. Rev., July, 1856, p. 74.

[4] Lancet, Aug. 26, 1854.

[5] Die Pathologie und Therapie der Rachitis, Berlin, 1863.

[6] Med. Times and Gazette, 1860.

[7] The part which the father plays in the transmission of disease seems, under all circumstances, to be very uncertain. Mr. Orton, in a paper on "The Physiology of Breeding" (quoted by Dr. Alexander Henry, *Edinburgh Medical Journal*, August, 1854, p. 113), has asserted "that diseases of the vital organs (and it may be presumed, therefore, diseases primarily involving the vital functions of nutrition and secretion), are transmitted oftener and in a more intense form and degree on the side of the mother than that of the father." This opinion is borne out by clinical experience in other diseases than rachitis. M. Cullerier (*Archives Générales de Medicine*, September, 1854, and *Edinburgh Medical Journal*, December, 1854), has come to the conclusion that syphilis is not transmissible from the father to his child, and Prof. Boeck (*American Journal of Syphilography and Dermatology*, Jan. 1870, p. 16) writes that the latter rarely inherits the disease except when the mother is affected. To this rule there are, we believe, occasional ex-

children to the affection. It is true that they will be born weak, but if properly cared for, and especially if fed from a full breast of milk, they will die of tuberculosis rather than of rachitis.

From these remarks it is apparent that we are not prepared to adopt the view that rickets is hereditary, though this opinion has been supported by some illustrious authorities. Herring[1] affirms that it is. Ritter von Rittershain's[2] observations seem to support the same opinion. Hennig[3] believes that hereditary influence is strong. Stewart,[4] Whitehead,[5] Merei,[6] Aitken,[7] and Willshire,[8] all believe in its hereditary transmission. Niemeyer[9] says that it appears to be so in some families. Vogel[10] says that "the possibility of its being inheritable is not to be ignored." Hillier[11] seems to favour the same opinion. Meigs and Pepper[12] believe that there is no evidence to show that it is ever hereditary. Holmes[13] is of the same opinion. Before this Gregory[14] had spoken more positively upon this point, saying that the constitution of the parents has little to do with the production of the disease; but the most positive authorities in regard to the matter are Routh,[15] E. Smith,[16] and Sir William Jenner,[17] all of whom deny that it is ever transmitted from parent to child. It will be seen, however, that the affirmative side of this question is supported by some able authorities, and that in point of numbers the advocates of this doctrine preponderate. Notwithstanding this the writer is acquainted with no facts which support their position, and his own experience has led him to believe that Smith, Jenner, and Meigs and Pepper are correct. He has several times seen women who presented indubitable evidences of having suffered from rachitis in early life, and who had borne large families, none of whom presented any manifestations of the disorder. One of the most

ceptions, though the conclusion which we have been forced to adopt is that it is much nearer correct than is generally supposed. Our own experience has furnished us with no facts which support Ritter's conclusion, while the analogy of syphilis is opposed to his doctrine.

[1] Hillier, Diseases of Children, Phila., 1868, p. 94.
[2] Ibid., p. 94.
[3] Lehrbuch krankheiten der Kinder, § 420, quoted by Willshire.
[4] Diseases of Children, 8vo., N. Y., 1841, p. 319.
[5] Hereditary Diseases, 8vo., London, 1851, p. 27.
[6] Disorders of Infantile Development and Rickets, London, 1855, p. 200.
[7] Reynolds' System of Medicine, London, 1866, vol. i. p. 768.
[8] British and Foreign Medico-Chirurg. Review, July, 1856.
[9] Practice of Medicine, New York, 1869, vol. ii. p. 509.
[10] Diseases of Children, New York, 1870, p. 532.
[11] Diseases of Children, Phila., 1868, p. 95.
[12] Diseases of Children, Phila., 1870, p. 634.
[13] Surgical Treatment of Diseases of Children, Phila., 1869.
[14] Elements of Theory and Practice of Physic, 8vo., Phila., 1829, vol. ii. p. 482.
[15] Infant Feeding, 12 mo., London, 1863, p. 443.
[16] Wasting Diseases of Children, Phila., 1870, p. 99.
[17] Med. Times and Gazette, 1860, vol. i.

remarkable of these presented striking deformities, and her labors had all been difficult; but, being a prolific woman, she is the mother of seven living children, all of whom are healthy, and none of whom have ever presented any traces of the disease. In our article in the January number of this Journal, it was stated, that, if the subjects of rachitis live beyond a certain age and recovery commences, it is perfect, and the patient is as likely to live to old age as one who has been perfectly healthy in infancy. This being the case, there is no reason for supposing that the disease can be transmitted from mother to child. Rachitis is strictly and only an affection of infancy, never commencing after it, always ceasing before puberty, and never directly modifying the health after that time. Jenner[1] doubtless reached the truth when he attributed the disposition of the disease to affect most or all of the children of certain families, to the ill-health of the mother, which interferes with her duties in nursing, or to the greater tax upon the parents which prevents them from properly providing for a large number of children.

In the study of the etiology of rickets, one point demands a passing remark. Hitherto it has been spoken of as a diathetic disorder, but, as Dr. E. Smith[2] justly says, it is not diathetic as struma, tuberculosis, and syphilis are. In other words, the child is not born with a predisposition to rickets, except so far as it is influenced by age. The sooner this point is granted and the sooner this is acknowledged to be an acquired affection the better it will be for those affected by it.

The true causes of the disease are to be found in the surroundings of the child. Moss,[3] who had much clearer ideas in regard to its etiology than many who lived before and after him, struck the keynote of the truth when he wrote, "By far the greatest number of cases which we see of it are chiefly or altogether brought on or increased by improper or bad nursing." Gregory[4] reiterates the same opinion. During latter times poverty has been looked upon as the great predisposing cause of the disease. This is directly contrary to the testimony of the early writers upon the subject. Glisson[5] said in his day that it "doth more frequently and vehemently invade the families of the wealthy than the cottages of poor men." Sir John Floyer,[6] who wrote that "no distemper is more frequent in children than the rickets," also said that it was a disease of the upper classes; and Boerhaave[7] evidently held the same opinion, for in his 1488th aphorism he says that it is generally more fatal to children whose parents are of a "weak

[1] Op. citat.

[2] Op. citat., p. 98.

[3] Diseases of Children, England, 1744, pp. 266 and 367.

[4] Elements of the Theory and Practice of Physic, 8vo., Phila., 1849., vol. ii. p. 482.

[5] A Treatise of the Rickets, 12mo., London, 1668, p. 213.

[6] History of Cold Bathing, 8vo., London, 1732, p. 76.

[7] Op. citat., p. 440.

and loose contexture of body, are idle, delicate, and live in a plentiful manner." This testimony is in direct opposition to that borne by most recent authors, with the exception of Billroth,[1] who states that "in Germany, it is about equally frequent in all classes of society." Long before this Rosen von Rosenstein[2] of Sweden had asserted of the disease in that country that it affected the richer and very poorest people, and that it was rarely seen among those of middling fortune. It is exceedingly probable that this remark is equally applicable at the present time. Wealth unfortunately does not bring knowledge in its train and fit a woman to perform the duties of a mother, while it too often furnishes her the means to delegate the care of her child to others—uneducated and unprincipled nurses—through whom it suffers from neglect, while the mother herself, leading a life of ease and idleness, furnishes milk which is poor in quality or deficient in quantity. Among these the disease rarely continues long before medical advice is sought, and hence among them only the mildest forms of it are now to be met with. On the other hand, the poor mother, inured to excessive labour, imperfectly supplied with food, and occupying badly ventilated apartments in a close quarter, could hardly help having rachitic children. Those in the middle classes, however, who perform a moderate amount of manual labor, which acts as a healthy stimulus; who from necessity attend to their children themselves; who are supplied with a sufficient quantity of food of a good quality, and who occupy comfortable homes, we would expect to have fewer rickety children than either of the other classes, and such we believe is the case.

These remarks show that the efficient cause of rachitis is improper feeding. West has said[3] that he has "never seen an infant while efficiently suckled by a healthy nurse or mother present any of the symptoms of rickets," and Lonsdale[4] uses equally strong language, for he writes, "In all rickety children parents have had little or no milk for their supply," and "a child will not suffer from this disease if the mother is healthy and the milk secreted is plenty and of good quality during the first twelve months of life." Merei,[5] on the other hand, thought that the character of the inspired air had more to do with the production of the disease. While we do not deny the importance of this, and while we believe that in some cases it is the efficient cause of the disease by its reactions on the digestive system, we do not think that it is so in the majority of instances. In by far the largest number of the cases which we have seen, the affection has had its origin in improper feeding. So firmly convinced am I of the potency of this cause that I should expect a child who was nursed by a healthy mother with a

[1] Surgical Pathology and Therapeutics, 8vo., New York, 1871, p. 452.
[2] The Diseases of Children, 8vo., London, 1776, p. 272.
[3] Diseases of Children, Phila., 1868, p. 589.
[4] Lancet, 1855, vol. ii.
[5] Op. Citat., p. 158.

full breast of milk to escape the disease, no matter what might be the character of the rooms which it occupied. On the other hand, no matter how well ventilated these are, or how sumptuously they may be furnished, if the infant is supplied with food poor in quality or deficient in quantity, it will be very likely to become rachitic.

The relation of rickets to the character and amount of food furnished opens the whole subject of infant feeding, and its discussion involves the consideration of the influences which unfit the mother's milk for the nourishment of her child, for it is in this indirect way that maternal ill health produces this disorder. Those causes of the disease which act directly upon the child have been generally recognized. In 1860 Jenner drew attention to ill health of the mother as one of the indirect causes, and remarked,[1] that, if she was the subject of anemia or general weakness, her children would be rickety, though the father was in good health, and the hygienic surroundings most favourable. In 1862 Dr. W. Tilbury Fox[2] gave considerable prominence to these indirect causes, and especially to the influence of menstruation of the mother during lactation. The tables which accompany his paper appear to be conclusive, but my own experience will not corroborate that of Dr. Fox, and I cannot believe that menstruation during lactation is by any means a fertile cause of the affection. I have met with quite a number of perfectly healthy children whose mothers menstruated regularly from the birth of their children until the latter were weaned at the end of a year or fifteen months. That menstruation during lactation will sometimes impair the quality of the milk we do not deny, and that it may thus become an indirect cause of rachitis we confidently believe; but that it is so general and certain in its operation as Fox believes is very doubtful. Because a nursing woman menstruates regularly, it does not necessarily follow that her child will be ill, and that it should consequently be weaned. In our own experience the largest number and best marked examples of rachitis have occurred in children who have not been wet-nursed at all.

Other discharges seem to be just as potent agents in the production of rachitis as the menses. I have sometimes met with women whose children I could almost certainly predict would become rachitic if they were wet-nursed. The mothers of such are usually pale, rather fleshy but flabby women, who present the external evidences of good health, but they are often the subjects of a more or less profuse leucorrhœal discharge, which either comes on spontaneously some time after labour, or continues from that time, the lochia gradually merging into it. I do not believe, however, that the vaginal discharge is the cause of rickets, but, on the contrary, that it is an evidence of ill health, and shows that the woman may not be in a condition to nurse her child. It is not every woman, who is

[1] Loc. Citat. [2] Trans. Obstet. Soc., London, vol. iv., 1863, p. 260.

the subject of leucorrhœa during lactation, who is incapacitated from wet-nursing.

In other cases the cause of rachitis has seemed to be pregnancy, which occurred during lactation. We have met with several examples of this kind. This does not seem strange, as the natural and well known physiological antagonism between the uterus and the mammæ, would render it almost certain that functional activity of one would be followed by functional quiescence of the other.

It seems to be demonstrated, therefore, that the most potent and frequent cause of the disease is the use of improper food; but it is to be remembered that this is not the sole cause. Long ago it was noticed that this affection might manifest itself after any acute disease which prostrated the child, and so Sydenham[1] and others described cases which followed acute diseases. This writer alluded particularly to malarial fevers. Indeed, it seems as if any child under one year old, who is depressed beyond a certain point, may become the subject of this disease, and Gee's[2] idea, that rickets is debility modified by age, does not seem so far wide of the truth. It is important to remember these facts, or the physician who looks upon improper food as the sole cause of the affection might commit the fatal error of weaning a child when its mother's milk was the most important and indeed the only curative agent required.

It has been stated that sex has some influence in the production of rachitis. M. Guerin[3] has asserted that the disease affects females more frequently than it does males. Out of 346 cases treated by him, 198 were girls, and 148 were boys. Beylard[4] admits the same proportion, and Brodhurst[5] says that out of 500 cases of the disease which he has tabulated, 326 were females, and 174 were males. These are the only authors who specially mention sex as a predisposing cause, and though their statements appear to be very positive, they must be accepted with great allowance, for without exception they have failed to recognize that the disease may occur in early infancy. Besides this, their experience is directly opposed to that of other observers, for among Ritchie's[6] cases the males exceeded the females. Out of 219 cases treated by him, 128 were boys, and 91 girls. According to the author's experience, sex has no influence in causing the disease. According to notes of 150 cases under his care, and in which the sex of the patient is mentioned, 78 were girls, and 72 boys.

Treatment.—It is important to remember that this is not a disease

[1] The Works of Sydenham. Syd. Soc. Ed., London, 1850, vol. ii. p. 100.
[2] St. Bartholomew's Hospital Reports, vol. iv. p. 78.
[3] Memoir sur Rachitis, Paris.
[4] Du Rachitis, &c., 4to., Paris, 1852.
[5] The Deformities of the Human Body, 8vo., London, 1871, p. 24.
[6] Medical Times and Gazette, July 7, 1871.

which has a certain definite course to run, but it is one which shows forcibly the good effects of well directed remedies. It is one which may be arrested at any stage in its progress, and one which, if properly treated, will nearly always end in recovery. As we have not hesitated to say that it is nearly always preventable, so we do not hesitate to assert that, if seen early, deformity should be the exception and not the rule.

It is not our purpose to discuss the necessity of furnishing the child with a plentiful supply of pure air and sunlight, nor do we intend to say anything in regard to the importance of supplying it with good food. The necessity for these is self-evident after what we have said.

Of the therapeutic agents for the cure of the disease, iron and cod-liver oil are the most important. The latter is almost a specific, and was long ago a common remedy for the disorder among the inhabitants of the shores of the Baltic.[1] The physician need not be deterred from giving it to these children on account of the diarrhœa from which they suffer: they will bear it when few other children will.

One of the best forms for the administration of this oil is an emulsion of it with the lacto-phosphate of lime, such as is made by Mr. Trinder of this city. More than a year ago the writer commenced the use of this preparation at the Philadelphia Hospital, and he has been entirely satisfied with the results. The preparation of Mr. Trinder is almost semi-solid, and each fluidounce contains half an ounce of Marvin's cod-liver oil and sixteen grains of the lacto-phosphate of lime.

Its advantages are that it can be given in cases where the pure oil will not be borne. Some children who were suffering from diarrhœa, and who rejected the ordinary oil, took this, retained it well, and improved rapidly. It could be given in the summer time, when the unmixed oil produced nausea and was rejected. What influence the lacto-phosphates have upon the course of the disease, I will not pretend to say, but I feel assured that patients improve more rapidly while taking this combination than they do when using the oil alone. M. Dusart[2] says that the lacto-phosphates do not precipitate in the stomach, but that they are absorbed and increase the amount of callus in cases of fracture.

I have repeatedly tried the other preparations of lime and the alkalies in accordance with the chemical theories of the disease, and have never found them to be of any use. A short time since, Dr. Sansom called attention to the good qualities of the sulpho-carbolate of lime in the treatment of the disease. This is a very soluble salt, but from the evidence of its usefulness furnished by its originator,[3] I would not exchange it for the lacto-phosphate and cod-liver oil, though it is but just to say that I have

[1] London Lancet, 1851, vol. ii. p. 546.
[2] Michigan University Medical Journal, July, 1871, p. 316.
[3] The Antiseptic System, etc., 8vo., London, 1871, p. 344.

never employed it. Dr. Ritchie[1] prescribed it in twenty-six cases, but only two of the children derived any benefit from it.

The most useful preparation of iron is probably the phosphate. If there has been long-continued diarrhœa, the solution of the per-nitrate is often preferable. The combination of cod-liver oil and iron is more efficient than either alone.

Quinia and other tonics are of course useful, and are to be administered in accordance with the usual indications.

The use of cold bathing is exceedingly important. The older writers on this disease speak of many cases cured by this agent alone.

The constipation of rachitic infants should not be treated by means of purgatives or enemata. It will often yield to cod-liver oil alone, but more certainly to the oil and iron combined. Where these have failed, I have sometimes, and with good effect, combined the iron and oil with manganese.

In the treatment of pseudo-paraplegia the nervous symptoms are to be ignored. These children improve rapidly under the ordinary remedies for rachitis. The same is true of the complications of the disease, laryngismus stridulus, and convulsions. Bromide of potassium is useful during the paroxysms and the intervals between them, to relieve the nervous irritability, but this remedy is not to be looked upon as in itself curative. He who ignores the local symptoms, and treats the systemic conditions upon which they depend, will prove the most successful in their relief.

The writer conceives that one of the most important questions in connection with the cure of this disease is the propriety of resorting to mechanical appliances to relieve the deformity of the lower limbs. About this there is considerable difference of opinion. As a rule, surgeons with one accord recommend the use of instruments, while physicians are either silent upon the subject or condemn them. Advocating the affirmative side of the question are Sir Astley Cooper,[2] Dupuytren,[3] Little,[4] Gross,[5] Coote,[6] Stanley,[7] Billroth,[8] Holmes,[9] and Meigs and Pepper.[10] Niemeyer,[11] Vogel,[12]

[1] Medical Times and Gazette, Jan. 7, 1871.
[2] Lancet, July 31, 1834.
[3] Diseases and Injuries of the Bones. Syd. Soc. Ed., London, 1848.
[4] On Deformities, &c., London, 1853, p. 215.
[5] Syst. of Surgery, 4th Ed., Phila., 1866, vol. i. p. 826.
[6] On Joint Diseases, London, 1867, p. 273.
[7] Diseases of the Bones, London, 1849.
[8] Surgical Pathology—Trans. by Hackley, New York, 1871, p. 453.
[9] Surgical Treatment of Children's Diseases, Phila., 1869, p. 347.
[10] Diseases of Children, Phila., 1870, p. 642.
[11] Prac. of Med., New York, 1869, vol. ii. p. 514.
[12] Diseases of Children, New York, 1870, p. 534.

and Spence[1] speak guardedly in relation to the matter, while Merei,[2] Jenner,[3] Hillier,[4] and Smith[5] oppose the use of splints for the cure of the deformity.

It is thus seen that the largest number of physicians are advocates of the use of instruments. Among these, Mr. Holmes Coote speaks very positively. He writes:[6] "The use of splints and apparatus is a subject of paramount importance, the object being to prevent alteration in the shape of the bones. If the child pass through this period of infantile suffering without great deformity, the recovery is complete. If the bones bend, the deformity is permanent." Mr. Coote has here fallen into a serious error. At the close of his paper in the January number of this Journal the writer called attention to the fact that these deformities sometimes diminish with surprising rapidity as the cure progresses. Any one who has seen many cases of rachitic distortion of the thorax must have been struck with the fact that, though extreme, this may almost entirely disappear. It has been argued that, because bow-legs are produced by the weight of the child's body, they will not spontaneously improve. This is by no means true, a fact which Lawrence announced long ago.[7] He saw children whose legs were much bent lose all deformity in a very short time, and he thought therefore that mechanical appliances to relieve these distortions were mischievous.

After a careful search of the literature of the subject, and the observation of many cases of the disease, the writer is convinced that there are no clinical facts to prove that the bending of the bones of the leg can be improved by mechanical appliances without inflicting other and more serious injury. From a study of the natural course of the disease, he can most positively confirm the statement made by Lawrence in 1829, though he is well aware that an adverse opinion is held by many competent surgeons, and he is strongly inclined to believe that an improvement which has followed the application of an instrument has occurred in the natural course of the disease, and has been erroneously credited to the appliance used. The iron braces which are often put on children in this city cannot be too strongly condemned. While they may give support to the softened bone, they must effect this by injuring and making pressure upon some other part of the osseous system, and this, in a vast majority of instances, is the pelvis, a part which is only too liable to be deformed when even the utmost care is taken.

Spence says[8] that "mechanical appliances should not be had recourse to

[1] Lectures on Surgery, Edinburgh, 1869, vol. ii. p. 280.
[2] Diseases of Infantile Development and Rickets, London, 1855, p. 218.
[3] Med. Times and Gazette, May 12, 1860.
[4] Diseases of Children, Phila., 1868, p. 116.
[5] Wasting Diseases of Children, Phila., 1870, p. 102.
[6] On Joint Diseases, London, 1867, p. 273.
[7] Lancet, June 5, 1829.
[8] Lectures on Surgery, Edinburgh, 1869, p. 983.

without great caution, and if used, should be constructed with the nicest care." He further states that they "may be necessary." We fully endorse the first assertion, but that splints "may be necessary" is a question which we conceive is still open for discussion. If it is answered in the affirmative, we do not hesitate to say with Boyer[1] that they are more useful to correct vicious positions than the deformities arising from the disease.

One of the advantages usually claimed for these supports is that they allow the child to take exercise while the constitutional treatment is progressing. Now, this is precisely what we do not want. Because the effects of the lesions of the bones are so palpable and to such an extent overshadow the other phenomena of the disease, rickets has been too often looked upon as a disorder of the osseous system alone. We have already shown that it is a constitutional affection, and that other tissues and especially the muscles are involved in the rachitic process. These are peculiarly weak, and children affected with the disease are indisposed to walk, a disposition which the physician is to foster rather than combat.

Passive exercise, by being carried or wheeled in the open air, is useful and necessary, but active movements of the child upon its feet should in most cases be prevented until the bones have acquired sufficient firmness to allow the patient to stand or walk without their bending. In some instances the improvement in the osseous system does not progress as rapidly as that of the muscles, when these children desire to exercise their newly acquired powers almost constantly. In these cases, as Jenner[2] suggests, light splints may advantageously be applied to the lower extremities and allowed to project beyond the feet, so as to prevent them moving about until the bones have acquired sufficient firmness to allow this to be done with safety. In other instances, as suggested by Boyer, they may be used in order to destroy a faulty habit of standing or lying. In both cases the splints are to be light, nicely adapted to the part, and are to be used *to prevent* and *not to cure* deformity.

This is the great secret in the mechanical treatment of rachitis. No external remedy will ever reach the seat of the disease, and, whatsoever may be its form, and however far it may have progressed, it can, with rare exceptions, be cured by the proper administration of cod-liver oil, lacto-phosphates of lime, iron, and quinia.

Some writers upon this subject have recommended moulding the limbs for the relief of the deformities, and Dupuytren has given it especial prominence[3] in the treatment of the deformities of the thorax which he describes. Since that time numerous examples of apparent benefit from it have been recorded in medical periodicals. The more carefully these are sifted the less they

[1] Diseases of the Bones, Phila., 1805, p. 196.
[2] Medical Times and Gazette, 1860, vol. i. p. 467.
[3] Diseases and Injuries of the Bones.

appear to support the views of the originator of the method. The physician has to separate the effects which naturally follow the process of cure from those of his manipulation, so carefully that it is very difficult to decide how much benefit is derived from the latter. It does not seem to have occurred to any that there is a physical impediment which prevents this manipulation from being beneficial. Jenner[1] has shown that the thoracic deformity of this disease is due to atmospheric pressure. The indication, therefore, is to permanently restore the relation normally existing between the capacity of the respiratory passages and the resistance of the chest wall. This cannot be done by merely re-expanding the collapsed sides of the thorax, but they must be retained there by counter-pressure of air within the lung. This can only be effected by the re-expansion of the collapsed portions of the latter.

The prevention of deformity is the most important part of the treatment of a case of well-developed rachitis, but that of the chest and extremities does not interest us so much as that of the pelvis, on account of the fearful influence of the latter upon parturition. One important point to be remembered here is, that distortion of the arms, chest, and legs bears no relation to that of the pelvis, and that the latter may be greatly deformed while these remain almost natural. The physician who believes that the pelvis becomes misshapen only from the weight of the superimposed body, will find himself grievously mistaken. The writer has several times met with lesions of the bones of this cavity, in which no bending or twisting could be detected during life, and in which the shape of the long bones of the upper and lower extremities remained nearly natural. The anterior projection of the lumbar vertebra may be due to sitting, in a child who has never walked, or the sides of the pelvis may be compressed so as to produce permanent deformity by a faulty position in lying.

For these reasons a child should be kept in a recumbent position and on its back until the pelvic bones become sufficiently formed to bear the weight of the superimposed body, and it should always lie on a firm and perfectly smooth mattress.

The craniotabes may require some special measures. These children tolerate pressure upon the occiput very poorly. Elsässer, therefore, says that the pillow of the child should be constructed with a large central aperture, which would remove the pressure from the occipital bone. The child usually obtains much relief from frequent bathing of the head with cold water.

The especial condition which is associated with the craniotabes, and demands treatment, is the cerebral irritation. To prevent this, everything which gives rise to congestion of the brain is to be avoided. Jacobi[2] says

[1] Medical Times and Gazette, 1860. [2] Am. Journal of Obstetrics, Nov. 1870.

that in cases in which there is passive congestion, it may be relieved by ergot. There is no better remedy in the irritability of these infants with a disposition to convulsions than morphia. It may be given fearlessly in proportion to the age of the patient. The writer is well aware that many physicians object to opium being given to infants, in which he cannot accord, and he has always prescribed this remedy whenever the same circumstances would have indicated it in the adult, and has never seen cause to regret it. Moreover, infants will often bear a dose larger in proportion to their age than adults.

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ART. II.—*A Review of the Recent Trial of Mrs. Elizabeth G. Wharton on the Charge of Poisoning General W. S. Ketchum.* By JOHN J. REESE, M.D., Professor of Medical Jurisprudence and Toxicology in the University of Pennsylvania.

THIS celebrated trial[1] has excited so much interest throughout the country, both on account of the high social position of the accused, and also from the nature of the evidence offered by the State, that we are persuaded a review of the scientific testimony—medical and chemical—cannot fail to prove interesting to our readers. The theory of the prosecution was, that Mrs. Wharton was in debt to Gen. Ketchum, and had invited him to her house in Baltimore for the purpose of poisoning him; and that she actually accomplished her design by means of tartar emetic.

For a better understanding of this, we will advert briefly to the facts of the case; first, however, calling attention to the very strong prejudice which seems to have prevailed in the public mind in the city of Baltimore against the accused, and which naturally induced her counsel to avail themselves of the merciful provision of the law, which permits the trial to be transferred to another county of the State. Accordingly, the venue was changed from Baltimore to Anne Arundel County; and the trial took place in Annapolis, on December 4, 1871. It terminated on January 24, 1872, after lasting for a period of fifty-two days; the jury bringing in a verdict of "*not guilty.*"

General Ketchum, the deceased, was an old army officer, aged fifty-eight years, an intimate friend of Mrs. Wharton, and of her late husband, Colonel Wharton. He was alleged to be in good health, and of active

[1] It may be proper to state that the materials for this Review have been taken partly from the writer's personal recollections, and partly from the Report of the Trial published by the Baltimore Gazette, in pamphlet form.

habits. On the 24th of June, 1871, a very hot, sultry day, he had undergone considerable bodily exercise in the city of Washington, by walking through the streets in the pursuit of his business matters. So much was he engaged in his occupation, that he neglected eating his dinner, although he partook plentifully of ice-water. He started for Baltimore later in the day, and arrived at Mrs. Wharton's house between six and seven o'clock in the evening. He partook of no food until supper-time, near nine o'clock. At this meal, he ate very heartily of various articles, among which was a plate of raspberries, remarking (according to one of the witnesses) that "he must make up for two meals." After supper, he sat up talking until about eleven o'clock, when he retired to bed, apparently in his usual health. During the night, he was heard to go down stairs once or twice, into the yard. The next morning, June 25th, he remarked that he had not been very well in the night; but he went out to visit a friend, after breakfast, and he partook of all his meals with the family, on that day; and although complaining of not feeling very well, he appeared brighter and more cheerful after tea, chatting and smoking with the family until about eleven P.M. Before going to bed, he drank a glass of lemonade, along with his friend Mrs. Chubb. His glass contained some brandy, which he put into it himself. On that night, he was again sick, as on the preceding night, having some slight vomiting and purging. The following morning (26th), he complained of a sick stomach and giddiness, and remained in his room during the day, although he partook of each of his meals. In the afternoon, at the strong solicitations of Mrs. Wharton and Mrs. Chubb, he consented to see a physician, and Dr. Williams was sent for. The doctor visited him about four o'clock, and found him sitting up, and vomiting into a slop-jar held between his knees. He ordered him to bed; and, regarding his case as an attack of cholera morbus, he directed him to take a mixture of lime-water and creasote every two hours. No mention is made of diarrhœa at this time. This simple treatment seems to have speedily relieved him, since, on visiting him on the following morning (27th), the physician found him quietly sleeping; and on being awakened, "he said he was much better, and expressed his determination to return to Washington that morning." He discharged his physician at that time, the latter regarding his patient as well.

Throughout this day, however, according to the testimony of several witnesses, he was more or less drowsy, requiring to be aroused, and seeming as if under the partial influence of some narcotic. Indeed, he remarked to the coloured woman who waited upon him, that "he had taken some of his own medicine, and that he knew that was sufficient; to let him alone, and he would sleep it off." This fact, taken in conjunction with the circumstance of a vial which had contained laudanum being found in his bed on that afternoon, leads strongly to the suspicion that the General had taken some laudanum on his own responsibility, for the purpose of relieving his sick-

ness. He is also described by the servant, as seeming to be very weak, so as scarcely to be able to walk.

No evidence of either vomiting or purging was discovered on Tuesday. Late in the evening of that day, Gen. K. was noticed lying on a lounge, "sleeping heavily, and breathing heavily." Mrs. Wharton having expressed her anxiety about her guest to Mr. Hutton, this gentleman went to Dr. Williams, and mentioned his condition to him. The doctor, however, did not judge it necessary to visit the patient at the time, but remarked that "he had been weakened by vomiting, and sleeping would do him no harm." On returning, Mr. Hutton found the General "turned upon his side, and breathing easily;" and so he was left for the night.

On the morning of the 28th, he was found still lying on the lounge, in an almost insensible condition. Mrs. Wharton at once dispatched a servant to Dr. Williams, at eight o'clock (the doctor says it was ten), with a request for him to come at once, as the General was much worse. He came soon after ten o'clock, and describes the patient as being "in a semi-comatose state, very difficult to arouse, and giving inarticulate answers." On being touched, "a slight convulsive tremor passed over him from head to foot." "His head and face were much congested, of a purplish tinge." On being raised up and supported towards the bed, "his arms and legs were found to be stiff and rigid." "He said nothing about his sufferings," and on being aroused, gave "only a muttered reply." His pupils were "not contracted, though insensible to light;" the respiration was about natural; the skin not preternaturally moist. The treatment consisted in the application of ice to his head, for the first hour; then he took forty drops of tincture of gelsemium in a little water, which appeared somewhat to diminish the congestion of his face. This last dose was directed to be repeated in the course of two hours. While the ice was upon his head, the patient made attempts to get out of bed; he had also some convulsions of a peculiar character, the tendency of which was "to throw the body from the back to the left side."

The doctor evidently considered him very ill, as he "remained with him two hours," and so informed Mrs. Wharton, and spoke with her of the propriety of telegraphing to his near relations. Still, the only treatment that was employed from ten to one o'clock was the application of ice to the head, at first, and the subsequent exhibition of two doses of tincture of gelsemium.

After taking the second dose of the latter, five minutes before one o'clock, the General began to be very uneasy, "first slapping the shoulder (of witness), as she sat by his bedside; then grasping with both hands the back of his neck, then the front of his throat, his stomach and his chest; uttering incoherent cries of oh! oh! don't! don't!" (*Vide* testimony of Mrs. Hutton.) The convulsions increased in intensity, giving to the body the true character of *opisthotonos*. The countenance and the movements of the

hands, the tearing at the neck, breast and abdomen indicated to one of the witnesses (Mr. Snowden) that "he was suffering excessive pain;" so much so, that he inquired of Mrs. Wharton "if the General had had any brain affection previously." These movements were, however, most probably of an automatic character.

These alarming symptoms at once led to the summoning of Dr. Williams, who reached the patient about half past one o'clock. He expressed surprise when informed that the General grew worse after taking the second dose of the yellow jessamine, and remarked that he "should have expected a similar result to that of the first dose." He very soon administered chloroform by inhalation, with a view of controlling the spasms, and also for the purpose of drawing off some urine by the catheter, that he might test it for albumen, as the idea of uræmic poisoning very naturally occurred to his mind. The test, however, gave him only negative results, and he abandoned that idea. Afterwards, thirty grains of hydrate of chloral were given in a little milk, but the convulsions, at the time, were so violent that "it was with great difficulty the spoon could be gotten into his mouth; he bit upon the spoon, so that his middle front tooth was loosened." No benefit appears to have been derived from either the chloroform, or chloral; and the General died about three o'clock, P.M., in a convulsion.

The *autopsy* was made on the following morning by Dr. Williams, assisted by Professors Miles and Chew. The brain and contents of the abdomen only were examined at this time. The brain is described by Dr. Williams, as "showing nothing that would explain the cause of death." "I discovered," says he, "numerous minute red points of a punctiform character, which were rather the effect of some other cause than that which resulted in death." "I cannot say whether they were ante-mortem or post-mortem." Prof. Chew states that "they found some evidences of congestion in the cerebral lobes; the congestion was punctiform, occurring in spots." Prof. Miles says that, although there was no congestion of the dura mater, on removing this membrane "there was the appearance of venous congestion in the other membranes." "There was no serous effusion under the membranes." "Throughout the substance of the brain were found those dark points of blood, which indicate passive congestion." The medulla oblongata presented nothing abnormal.

As regards the viscera of the abdomen, the alimentary canal is described by Prof. Chew as showing "some evidences of congestion, but not very well marked, or unmistakable;" they were not considered as "very significant." Prof. Miles found the intestines "congested in patches," and "containing a whitish, pulpy substance along their track." The liver, spleen, and kidneys were in a normal condition. The stomach, after being properly secured by ligatures, was removed, and taken to Professor Aikin, of the University of Maryland, for chemical analysis. When this organ was

subsequently opened, there was "some slight congestion about the lower part of it," according to Dr. Chew; and according to Dr. Aikin's Report, "nothing of a very marked character," pointing to inflammation.

Prof. Aikin, in due time, reported, as the result of his analysis of the contents of the stomach, that "he had satisfied himself" of the existence of tartar emetic in that organ, and in the quantity of "at least twenty grains." His *method* of arriving at this conclusion we shall discuss hereafter. At present, we refer merely to the fact of his analysis and his alleged results as forming the probable ground of the opinion of Dr. Williams, and of Professors Chew, Miles, Donaldson, Howard, Johnston and Smith, that "the death was not due to natural causes." It is true that some of these gentlemen testified to a hypothetical case put to them by the State; but one of the very strong elements of this hypothetical case was "the supposed finding of twenty grains of tartar emetic in the stomach of the deceased." Hence, we may reasonably infer that the opinion of these medical witnesses was materially influenced by the *assumed* fact of the existence of the poison in the stomach of General Ketchum, at the time of his death. If this one element had been left out of consideration, and their minds had been entirely free from the bias which such an impression must necessarily have imparted, we cannot but believe, from the tenor of the testimony given by these same witnesses, that they would not have entertained the opinion that the death had resulted "from non-natural causes."

Let us see whether the facts of the case, as brought out by the evidence, do not sustain us in this belief. Certainly, the attending physician, Dr. Williams, did not suspect that his patient was labouring under the effects of any poison, at his first visit, on the afternoon of June 26th, for he tells us distinctly that he regarded the case simply as one of "cholera morbus;" and states, moreover, that Gen. K. told him on that occasion "that he had had cholera morbus on Saturday and Sunday nights" previous. He further states, in his cross-examination, that "if tartar emetic had been present" at this time, "the creasote prescribed by him would have had no effect." This was, of course, a correct conclusion, and goes to establish the proof that, at this time at least, there was no suspicion of poison in the mind of the attending physician.

It would appear, however, from the evidence, that both the patient and the physician were mistaken as to the true condition of the former on Tuesday morning (27th), since, as already noticed, he was in reality suffering from a sort of narcotism—being in a more or less drowsy condition throughout the entire day.

The next occasion of Dr. Williams being called was on the 28th, about ten o'clock A.M. He had been again summoned by Mrs. Wharton to see Gen. K., who was worse on that morning. His condition, on that occasion, has already been described. There was not the least sign of irritation of the stomach or bowels; neither nausea, vomiting, nor purging; no

characteristic relaxation of tartar emetic, but on the contrary, a decided rigidity of the arms and legs, and clenching of the jaws. Most assuredly, in this stage of the disease, poison does not seem to have been suspected, since no means were taken either to evacuate it, or to administer any antidote. Indeed, Dr. W. appears (quite naturally) to have been at a loss to account for the condition of the patient. At one time, his "impression was that he had congestion of the brain, threatening apoplexy;" then, "dismissing this idea, he feared paralysis;" then again, when informed of the discovery of a laudanum vial which fell from the General's bed on the preceding evening, entertaining the impression of "opium poisoning, though not to a dangerous extent;" but subsequently rejecting this idea, on account of "the natural size of the pupils and their insensibility to light," the absence of "slow and laborious breathing," and the general rigidity of the muscles. Finally, he "feared uræmic poisoning," and drew off some urine by the catheter, and tested it for albumen, but "found it perfectly healthy."

The only treatment all this time directed was "the application of ice bags to the back of the neck" for one hour; then (at eleven A.M.) the administration of "forty drops of tincture of gelsemium in two teaspoonfuls of water." As the result of this dose, the patient's appearance is stated "to have improved; his color was better, and the appearance of the eye notably better." This dose was directed to be repeated in two hours. During the interval, the patient appears to have shown "some little twitching of the fingers" (*Vide* testimony of Mr. Snowden); and "he looked very much like a dying man," between twelve and half-past twelve. (*Vide* evidence of Mr. Loney.) And Prof. Smith testified that he "considered that the party described was dying at eleven A.M."

The second dose was administered by Mrs. Wharton herself, in the presence of Mrs. Hutton and Mr. Snowden, and with the aid of the latter, a few minutes before one o'clock. These two persons testify that Mrs. Wharton seemed unduly anxious about the arrival of the time for giving the medicine, frequently inquiring about the hour, and actually preparing the draught before the designated time. They assert also that the liquid administered by her was greater in quantity, and darker in colour than the former dose. Now, we cannot suppose it possible that any suspicion of poison had at this time entered into *their* minds, since they offered no opposition to the administration of the draught, and gave no alarm.

Again, on the third visit of Dr. Williams to Gen. K.—about half past one o'clock the same day, he entertained no more suspicion of poisoning than on the two former occasions, since he gave no emetic, used no stomach-pump, nor exhibited any antidote. If any doubt arose in his mind, it was possibly that the gelsemium had disagreed with him. At all events, *he adopted no means whatever to counteract any suspected poison*, but simply employed the ordinary means for the relief of convul-

sions—chloroform and chloral. Indeed, he tells us in his testimony, that the suspicion did not enter his mind, until after he had tested the urine, and about an hour before the patient's death.

But just at this crisis, an event occurred which was calculated to make an impression on the doctor's perplexed mind. A suspicious-looking tumbler of milk-punch had lately been discovered in the dining-room, by some of the numerous visitors at the house. This punch had been prepared some hours previous, for Mr. Van Ness (who was lying sick in the parlor of the same house), by his own wife and sister; and he had partaken, at the time, of a portion of it. When the wife, some time afterwards, returned to the dining-room for the balance of the punch, on pouring it out of the tumbler, she noticed a white sediment at the bottom, which imparted to her tongue a disagreeable, metallic taste. This tumbler was carefully preserved, and was at once carried off by one of the sisters to several places, in order to get it tested, but without success. It was shown to Dr. Williams soon after his arrival at the house—about two o'clock.

In the excitement of the household, this alleged discovery is made to play an important part in the drama of Gen. Ketchum's death. It would seem that the wavering and conflicting theories that had perplexed the mind of the physician, as to the nature of his patient's disease, now assumed a definite form, in the shape of tartar-emetic, or some other poison. He communicated his suspicions, that same evening, to Gen. Brice, brother-in-law of Gen. Ketchum; and he made the post-mortem examination on the following morning, removing the stomach for chemical analysis. The other viscera of the abdomen, together with the brain, were examined at the same time. Two weeks subsequently, the body was exhumed, and the thorax was examined. The spinal cord was not examined at all.

Let us now advert to the medical testimony for the prosecution. The physicians here summoned were gentlemen of ability and reputation, Professors Chew, Miles, Donaldson, Howard, Johnston, and N. R. Smith—all colleagues of Prof. Aikin, in the University of Maryland—together with Dr. Williams and a few other non-professors. Dr. Williams appears to have been specially retained by the State, and he evinced much zeal in working up the case. He tells us (as already noticed) that the suspicion of poison did not enter his mind until about an hour before his patient's death; that after the discovery of the suspicious tumbler of milk-punch in the dining-room "his doubt was very much changed into a conviction that he had been poisoned." It should be here observed that the tumbler of punch had no connection whatever with Gen. Ketchum; it had been prepared for another sick person, in an entirely different part of the house, by the wife and sister of that person, and there appears not the slightest evidence to connect it in any manner with either Mrs. Wharton or with the deceased.

Still, Dr. Williams "suspected that he had died from poison anterior to the post-mortem examination;" and "by the evidence of the post-mortem,

his previous opinion was strengthened." "He knew of no instance in which yellow jessamine had produced convulsions" (though such cases have been reported). "If he had known that Gen. Ketchum was under the influence of tartar emetic, he would have given him the yellow jessamine." "He does not admit that tartar emetic is a depressant," nor "would he say that yellow jessamine is a depressant." He also testified that "he had never seen a case of tartar-emetic poisoning;" and "that tartar emetic was more soluble in tea than in water." He also testified that "he had failed to discover in the lungs, brain, heart, or liver any evidences of death from other than natural causes."

Prof. Chew, of the University of Maryland, testified that he had been a practising physician thirteen years; "that from the narrative of symptoms as given by Dr. Williams, and from his own observations of the changes in the organs, he should say the case was a very obscure one, and he could not assign a cause for death." Still, although this gentleman admitted that, judging either from the symptoms alone, or the post-mortem alone, he could not ascribe the death to a non-natural cause, nevertheless it was his opinion, judging from both combined, "that he did not die from natural causes." He further stated "that his opinion to this effect was formed from the fact that at the post-mortem they found in no organ such changes as would have produced death." We would simply ask here, if the above is a logical inference?

Prof. Miles, of the University of Maryland, next testified that "he had been a practising physician twenty years; that he had no experience in cases of poisoning with tartar emetic;" and "that he had never made a post-mortem examination in a case of death from tartar emetic;" that "he could not say, from the symptoms alone, what was the cause of death; the negative evidence of the post-mortem examination would not alone be sufficient for him to base an opinion upon as to the cause of death." And yet, in apparent contradiction to the above, he testified further, that, "from the narration of Gen. K.'s symptoms, and from what he saw at the post-mortem examination, his opinion was that he did not die from natural causes." We would again ask the question, Is the above a logical conclusion?

Prof. Donaldson, of the University of Maryland, has been in practice twenty years; he testified as an expert, to the hypothetical case put by the State, that "he would be very much puzzled to make a clear diagnosis from the description given; there are points of resemblance to several well-known diseases, but at the same time, there is an absence in each of certain prominent symptoms." He alluded to cholera morbus, uræmia, apoplexy, congestion of the brain, tetanus, and cerebro-spinal meningitis, as the diseases which, in some of their features, might be suggested by the symptoms presented by Gen. K.'s case, and by the hypothetical case. He thought, however, "that the absence of the characteristic post-mortem lesions pre-

cluded the idea of death from cerebro-spinal meningitis." He, therefore, "felt justified in saying that, to the best of his knowledge and belief, he did not die of natural causes." He "has never seen a case of acute poisoning from tartar emetic;" but he thought that the symptoms described "closely corresponded to what the authors state." [Among the symptoms referred to by the witness, no mention is made of the most prominent ones of tartar emetic poisoning, viz.: vomiting, purging, extreme relaxation, insensibility of the skin, and increase of the urinary secretion—*all* of which were absent in Gen. K.'s case.] He admitted that "he would not have given an opinion as to the cause of death only on the symptoms;" and "he should have declined to have given an opinion from the post-mortem observations alone." He further testified that, although "there are diseases in which there are no perceptible post-mortem lesions," yet "it is from the symptoms and the post-mortem observations that I am enabled to arrive at an opinion." [It will be remembered that the post-mortem observations were mostly of a negative character.] He further testified that "a semi-comatose condition is not mentioned by the authors as one of the symptoms of tartar-emetic poisoning; and that insensibility, but not coma, is mentioned."

Prof. N. R. Smith, of the University of Maryland, has been practising medicine for fifty-two years; testified as an expert, on the hypothetical statement, that, according to the best of his judgment, "the party therein described died from non-natural causes." He was "familiar with the sporadic form of cerebro-spinal meningitis, especially in children." "The yellow jessamine, chloral, and chloroform were not such agents as I should have prescribed; they were depressing agents, and inappropriate; but I do not think they had any effect in determining the result." Again, "I consider that the party described was dying at eleven A.M. on Wednesday."

Prof. W. T. Howard, of the University of Maryland, has been in practice since 1844; testified as an expert, on the hypothetical statement, that "he did not think the party therein described died of cerebro-spinal meningitis," "finding no adequate lesion after death to account for the symptoms," and also judging from "the clinical history of the case recorded in that hypothetical statement."

Prof. Christopher Johnston, of the University of Maryland, has practised medicine about twenty-five years; testified as an expert, that "in his opinion the party therein described did not die from cerebro-spinal meningitis;" and, "as that picture presents no symptoms with which I am acquainted, I should infer that he died from non-natural causes."

The argument of these several witnesses seems to be that, because all the symptoms of the hypothetical case did not, in their opinion, correspond with certain diseases, and also because the generally recognized lesions of these diseases were not observed after death, *therefore* the death must have resulted from a non-natural cause, *i. e.* poison. This is virtually to admit

that poisons (and the question here is only with the *metallic* or *irritant* poisons) must prove fatal, without ever leaving any lesion behind, discoverable by post-mortem examination!

Dr. Geo. W. Benson, a practitioner of Baltimore for twenty years; testified as an expert, to the hypothetical case, that "he did not think the party therein described died from cerebro-spinal meningitis," but "from non-natural causes."

Dr. Girard E. Morgan, a physician of Baltimore, has been practising for twenty-one years; testified as an expert, to the hypothetical case, "that he had seen but one case of cerebro-spinal meningitis;" "that he could hardly give a direct answer to the inquiry;" but "he had never seen such an assemblage of symptoms from a natural cause." "He had heard of other cases of cerebro-spinal meningitis occurring in the city about the same time."

Dr. Chas. H. Ohr, a practitioner of thirty-eight years, of Cumberland, Md.; testified to the hypothetical case, as an expert. "He had had some experience with cerebro-spinal meningitis, but he did not think that there were any symptoms of this disease in the hypothetical case described." In his opinion, the party there described "did not die from a natural cause." This witness subsequently corrected himself, saying, "that he did not wish to be understood as meaning that none of the symptoms of cerebro-spinal meningitis were present in the hypothetical case; but only none which were distinctly characteristic;" he did not think that there is any pathognomonic symptom of cerebro-spinal meningitis, as he understood the disease.

Dr. Abram P. Arnold, a physician of Baltimore, in practice twenty-two years; testified as an expert, to the hypothetical case, that he was somewhat familiar with cerebro-spinal meningitis; that "the symptoms stated in the hypothetical case did not correspond with what he knew to be the symptoms of cerebro-spinal meningitis;" that "he was not prepared to give a direct answer to the interrogatory appended;" and "that in his opinion, the set of symptoms which preceded the death of the party described was of a very suspicious character."

It should be noticed that none of the medical witnesses for the State testified that tartar emetic was the cause of death. Dr. Donaldson said that "he thought the symptoms resembled what the authors stated to be those of tartar-emetic poisoning." All the others merely spoke of the death as being produced by "non-natural causes."

The medical testimony of the defence will next claim attention.

Dr. John J. Reese, of Philadelphia, was the first medical expert called. He was also examined as a chemist. He testified that he had been practising medicine over twenty-five years; "that there are no symptoms which are exclusive to tartar-emetic poisoning;" that "if a poison could be recognized by its symptoms alone, it would not, of course, be necessary

to make a chemical analysis;" that "there are many diseases whose symptoms closely resemble those of irritant poisons, as cholera morbus, especially in its initial symptoms, gastro-enteritis, peritonitis, and some others." He had seen one case of tartar-emetic poisoning which, however, was not fatal; here "there were great muscular relaxation and pallor of the face." "He would not pronounce this a case of tartar emetic poisoning from the symptoms alone;" "from the testimony of Drs. Williams, Chew, and Miles, he should not form the opinion that Gen. K. died of poisoning, judging from the symptoms and from the details of the post-mortem combined; and for the reason, that such symptoms and such post-mortem signs might be logically accounted for from natural causes." He further stated that "among the natural causes or diseases to which the death might be logically attributed, was one disease indicated by Prof. Donaldson—cerebro-spinal meningitis; that many of the symptoms in Gen. K.'s case strongly resembled those of the above-mentioned disorder, especially the pain in the back of the head and neck, the pain extending round the abdomen, the lividity of the face, the suppression of the urine, the extreme sensitiveness of the skin, the rigid contraction of the muscles of the neck, the bending back of the body in the arched form (*opisthotonos*), the partial delirium, and the condition of the eye; also the accompaniment of nausea, especially in the earlier stages." He further testified "that, in exceptional cases, cerebro-spinal meningitis (especially the fulminant form) may prove fatal, without leaving behind any discoverable lesions. This happens also in other diseases of a convulsive character, as hydrophobia, epilepsy, and some others."

The witness further stated, that the failure to examine the spinal cord and its membranes left it uncertain how far this organ might have been affected by disease; "that in all medico-legal investigations the rule was that the examination should be thorough and exhaustive, omitting no portion of the body." He also said "that he did not wish to be understood as positively asserting that Gen. K. died of cerebro-spinal meningitis." "He thought the cause of his death obscure; he meant to say that there was a probability that he died of this disease, and that he did not die from poison." Also, "that he did not consider tetanus as answering to the symptoms of the deceased as closely as did cerebro-spinal meningitis." Dr. Reese cited, as authorities sustaining his position in reference to the occasional absence of all pathological lesions in certain forms of cerebro-spinal meningitis, Aitken, Reynolds, Tanner, Niemeyer, Radcliffe, and Flint.

Dr. Edward Warren, a physician of Baltimore, practising for nearly twenty-two years, next testified, as an expert; that "he could see nothing in the symptoms described in Gen. K.'s case, or in the revelations of the post-mortem examination, or in them both combined, inconsistent with the operation of natural causes in the production of death."

This witness gave a very graphic and lucid description of the symptoms

and progress of cerebro-spinal meningitis, as noticed by himself, and as detailed by the standard authorities (the same authorities as cited by the preceding witness). He considered the identity of the symptoms in Gen. Ketchum's case with those of cerebro-spinal meningitis as being "complete." This was shown "by the suddenness of their development, their violence, and the rapidity of their course;" by "the condition of semi-consciousness, the increased sensitiveness of the surface, so that the patient shivered when touched; the rigidity of the muscles of the neck, back, and inferior extremities; the pupils neither contracted nor dilated, but insensitive to light; suppression and retention of urine; rapidity and compressibility of pulse; lividity of countenance; trismus; opisthotonos; disposition to turn on one side involuntarily; occasional jactitation and restlessness; incoherent articulations, and a speedy and violent death." The absence of pathological lesions was stated by the witness to be of occasional occurrence in the "fulminant" form of the disease; and, to support this assertion, he quoted several recognized authorities.

The witness further testified that, in his opinion, Gen. K. could not have been poisoned with tartar emetic on Saturday night, or Sunday night, on account of the length of time (several hours) that elapsed between his eating and drinking for the last time, on those occasions, and the manifestations of his slight cholera-morbus symptoms; again, that he could not have been poisoned on Monday, because, when visited by Dr. Williams on that day, he was sitting up "with the slop-jar between his knees"—a degree of muscular power incompatible with the extreme prostration uniformly attendant on antimonial poisoning; also, that he was relieved by creasote and lime-water, which are not antidotal, either chemically or physiologically, to tartar emetic; again, that he could not have been poisoned by tartar emetic on Tuesday, because his symptoms on that day were altogether incompatible with all the recognized symptoms of antimonial poisoning; and finally, he could not have been so poisoned on Wednesday, both because of the absence of the well-marked tartar-emetic symptoms, and still more because of the remarkable identity of his symptoms with those of cerebro-spinal meningitis. The witness expressed it as "his opinion, that no other theory will explain the history of his sickness and death; nothing else will adequately account for all the facts of the case." He likewise stated, that the negative character of the post-mortem signs negatived the theory of antimonial poisoning. He considered "the most important and invariable symptoms of this to be nausea, vomiting and purging, sedation of the heart's action, as indicated by a feeble, frequent and compressible pulse, disturbance of the respiratory function, together with the most profound muscular prostration, cramp, or convulsive movement in certain groups of muscles, a cold, clammy skin, copious secretion and discharge of urine, and a constant disposition to syncope, with venous congestions."

Dr. John Morris, a practising physician of Baltimore for nearly twenty-six years, next testified, as an expert, to the hypothetical case, "that it was a difficult and delicate question; that he could not assign a cause of death, if simply asked that naked question; that he saw nothing to exclude death from a natural cause, but, at the same time, he could not venture to say what that natural cause was." The witness described two cases of cerebro-spinal meningitis, that were seen by himself and Dr. Warren, and stated that the disease was quite prevalent that season in Baltimore. "He had never made a post-mortem examination in a case of that disease." Among the symptoms he described, he stated his belief that "the cries" of the patient did not necessarily indicate pain; and his opinion was that "the movements are more automatic than otherwise."

Prof. Harvey L. Byrd, of Washington University, Baltimore, in practice for thirty years; testified as an expert, to the hypothetical statement, that, as the result of his reflections thereon, his opinion was "that Gen. K. had died from natural causes, *i. e.* from disease, though he was embarrassed to decide what particular disease." "In some features, it resembled cerebro-spinal meningitis." "The suppression of urine constitutes an almost unfailing symptom, in certain conditions of this disease; but it is not so in tartar emetic poisoning." The witness regarded the symptoms, on the whole, as more closely fitting cerebro-spinal meningitis than any other disease.

Dr. Peter Goolrick, a physician of Baltimore of twelve years' standing; "had heard the testimony of Drs. Williams and Chew, and read that of Dr. Miles. From the symptoms and post-mortem revelations, he would say that Gen. K. might have died from natural causes, and not from tartar emetic." "Several diseases might produce the symptoms described." "There are several diseases which leave no lesions, and it would be unsafe to say that they were caused by tartar-emetic poisoning." "The tetanic spasms in tartar-emetic poisoning are a divergence from the usual symptoms."

Dr. John R. McClurg, a practitioner of medicine since 1846, now residing in West Chester, Pa.; testified to the hypothetical case, that "he had no hesitation in saying that death was the result of natural causes; it resembles, undoubtedly, cerebro-spinal meningitis more particularly than any other disease." He had treated "certainly fifty cases" of this disease "in private and military practice." He considered it "impossible for a poisonous dose of tartar emetic to have remained in the stomach from half past eight A.M. until half past twelve P.M. (Wednesday), without producing the characteristic symptoms of tartar-emetic poisoning." The witness "had seen probably twenty post-mortems of cases of cerebro-spinal meningitis; the lesions are very various;" "at times, we have been able to find no marks of inflammation, but only a congested condition of the bloodvessels and membranes of the brain, but without effusion." "The

brain-substance may be poisoned (in cerebro-spinal meningitis), and no inflammatory lesions be left." "The suppression of urine is one of the characteristic symptoms of the disease." "If antimony had been found in his stomach, liver, and kidneys, I could not say whether he died from antimonial poisoning, cerebro-spinal meningitis, or apoplexy from congestion." The witness further testified, that he was acquainted with the effects of gelsemium, and that he frequently used it in his practice; but that "he did not think that the (apparent) change for the better, after eleven o'clock on Wednesday, was at all attributable to it." He further "did not think that, if tartar emetic had been given to him at one o'clock (on Wednesday), it would have had any effect, for he was too far gone."

Dr. Wm. H. Baltzell, a practitioner of Baltimore of thirty years' standing; testified to the hypothetical statement, that the death "was the result of natural causes." "From his experience with a great many cases of cerebro-spinal meningitis, he would say that it most resembles that disease." The witness related a well-marked case of this disorder occurring in Baltimore in May last, to which he called Dr. Warren in consultation, all the symptoms of which remarkably resembled those of Gen. Ketchum. [This case was also alluded to by Dr. Warren, in his testimony.] He also stated that other cases of a similar character had occurred in Baltimore, about the same time. "He had seen cases of tartar-emetic poisoning; a fatal case in a child from an accidental dose, the quantity unknown; the muscular system was perfectly relaxed; the skin was moist and insensitive; no impression was made by handling; the circulation was very feeble; the action upon the kidneys was powerful, by increasing the secretion. This child died from excessive vomiting and purging; but from appearances, the case might have been mistaken for either Asiatic cholera, or cholera infantum."

Dr. Josiah Simpson, Surgeon U. S. Army, commissioned in 1837; was examined on the hypothetical case; and from it saw "no indication of death from unnatural causes;" his "conclusion was that the death was from natural causes;" he saw "nothing in the symptoms, or the post-mortem examination, to justify him in any other conclusion." "He had witnessed the effects of an overdose of tartar emetic in others, and in himself; it completely prostrated him; he could not raise his head to vomit," and "the complete prostration resulting lasted for more than twenty-four hours."

Dr. Abram Claude, a physician of Annapolis, in practice for thirty years; "heard the testimony of Drs. Williams, Chew, and Miles; would, from Gen. K.'s symptoms and the post-mortem revelations, ascribe his death rather to a natural, than a non-natural cause;" he "was acquainted, to some extent, with cerebro-spinal meningitis, having seen a good many cases in 1862, and also some since." Among the most characteristic symptoms, he enumerated "rigidity of the muscles of the neck and back, throwing back

of the head, hyperæsthesia of the skin, and insensibility of the pupils." He was familiar with the effects of tartar emetic, having used it in practice for years. "Had never seen a fatal result from its use. He had often tasted it, and it had never produced any soreness about his mouth or tongue." "A man may die from natural causes, and the post-mortem fail to reveal them; this is true particularly of diseases affecting the nervous apparatus." The witness further stated, that he considered the clutching movements of the hands, in Gen. Ketchum's case, "did not necessarily indicate pain;" he did not consider him to have been in good health at the time of his arrival at Mrs. Wharton's house, "as he was then taking medicine, by the advice of his physician in Washington." He moreover "thought, from the symptoms described at ten o'clock A.M. on Wednesday, that it mattered very little what was done for Gen. K. after that time; he considered him then to be labouring under a mortal disease, which steadily progressed up to the time of his death." "The last dose did not have any important effect, if any at all, in hastening the termination of the case." He "supposed that any poison might cause convulsions." "He would not undertake to say positively and decidedly what was his disease;" neither had he said positively that Gen. K. had "died from cerebro-spinal meningitis."

We next turn our attention to the chemical testimony. This, of course, constituted the main feature of the case. The finding of a poison (except in very minute quantities) in the stomach, or tissues of a body, is always strong presumptive evidence that death had resulted from the poison. If, in addition to this discovery of poison, the symptoms before death were characteristic of that particular poison, then the conclusion might be considered almost positive that the poison was the cause of death. The reason why we at all qualify the remark is, that *it is possible* for poison to be introduced into the stomach (but not into the tissues) after death, in a case of disease, where the preceding symptoms strongly resembled those characteristic of the poison itself. But such a case must necessarily be of very rare occurrence.

Dr. William E. Aikin, Professor of Chemistry in the University of Maryland, for thirty-five years; testified to receiving from Drs. Chew and Miles, the glass jar containing the stomach of Gen. Ketchum, on the 30th of June, 1871. From the information given to him by the medical gentlemen, he was led to infer the possible existence of "strychnia," or of "arsenic" in that stomach. He tested for the former of these by the process of Stas, which gave him a negative result. He next proceeded to examine for the inorganic poisons, arsenic and antimony, using the material already employed in the analysis for strychnia. This residue he divided into two portions, A and B. The portion A was treated with strong hydrochloric acid and chlorate of potassa, by heat; afterwards a stream of washed sulphuretted hydrogen gas was transmitted through it;

the precipitate thus obtained was of a "dark brownish colour." Finding this precipitate to be insoluble in aqua ammoniæ, he "was satisfied that it could not be arsenical." He then experimented with portion B, being led "to suspect that antimony was present," from the reactions of portion A. He testified that "there could not have been present (in portion A) any metal which forms a dark, or black sulphide, or the precipitate would have been black." But he admitted, on the cross-examination, that "lead would sometimes give a red precipitate, which might be mistaken for the antimonial red."

The witness further testified that "the presence of antimony is established by obtaining two or three results, which he called characteristic; these results would be: the orange-red sulphide of antimony; that this orange-red is soluble in strong, boiling muriatic acid; that acid solution dropped into water, gives a white precipitate; this white antimonial precipitate is made orange-red by sulphide of ammonium." Any substance which would give all the above results "must contain some antimonial;" and, as "he got all these results from the portion which he examined, he could not entertain the shadow of a doubt as to the presence of antimony."

In testing portion B for antimony, he "added an excess of tartaric acid, filtered it, and examined the filtrate with sulphuretted hydrogen." He deemed "there was no necessity for destroying the organic matter." The precipitate thus obtained was "reddish-brown, or brownish-red." When this was "separated and dried, it dissolved in muriatic acid; this solution, when dropped into water, gave a white precipitate; that white precipitate became orange-red, when treated with sulphide of ammonium; and it was soluble in tartaric acid. This completed all that was necessary to satisfy me of the presence of antimony; I know nothing that would have produced these results except some preparation of antimony."

The witness further testified that, in his opinion, "the quantity (of tartar emetic) present in the stomach could not have been less than twenty grains or more." His method of arriving at this conclusion is certainly novel, if not unique, in chemical determinations. It is to be remembered that the Professor passed sulphuretted hydrogen through the solution obtained from the stomach, strongly acidified by tartaric acid, *but still containing all the organic matter*. Now, it is well known by every chemist that, under such circumstances, a precipitate, more or less copious, will be thrown down, of a "reddish-brown" or yellowish-brown colour, which may be composed *exclusively* of sulphur and organic matter. If there should be any metal present, then the precipitate would contain the metallic sulphide, mixed with a variable quantity of sulphur and organic matter. Now, Prof. Aikin assumed that his precipitate, thus obtained, consisted exclusively of sulphide of antimony, *when, for aught he knew, it might have contained only sulphur and organic matter*. But, stranger

still, he did not even think it necessary to employ the ordinary method of *weighing* his precipitate; he was content to compare it, by the eye, with another precipitate "obtained from the sediment of the tumbler given him by Dr. Chew." The novel method of getting at the quantity of this second precipitate should not be passed by unnoticed. "I weighed out," says he, "one grain of the white sediment from the tumbler" (supposed by him to be tartar emetic); "the antimony obtained was in the shape of sulphide of antimony; and that, dried and weighed, was four-tenths of a grain; from which I inferred that the tartar emetic present in that grain of sediment must have been eight-tenths of a grain. If that was true of one grain, eight-tenths of the sediment must have been tartar emetic." But what was the actual amount of the sediment? Surely, it was easy enough to weigh it; this, however, was not deemed necessary by the analyst. He adopted a simpler method—*he guessed at it!* He tells us: "In my judgment, fifteen grains were, speaking within bounds, present; and the tartar emetic I put at ten grains, desiring to underestimate rather than overestimate." He then goes on to compare, *by his eye*, the bulk of the precipitate obtained from the one grain of the sediment, with the bulk of the precipitate obtained from the solution of the stomach. This latter precipitate, we have just shown was made up largely, if not exclusively, of sulphur and organic matter; and such a mass, when dried, might have a very considerable bulk, and yet possess but very little weight. Still, it is by this very extraordinary method of comparison, that Prof. Aikin undertakes to assert that he had discovered "twenty grains of tartar emetic in the stomach" of Gen. Ketchum!

Prof. Aikin admitted that he "did not use all the processes by which antimonial poisoning may be detected;" he "used means by which he arrived at what he had a right to consider a perfect certainty;" these means "he had learned when a student, and they were then recognized as reliable." "He had been teaching these methods to his classes so many years, that he could not remember when he began to teach them." He admitted that he had no *results* of his analysis to produce in court; "he presumed he might have gotten metallic antimony;" *but he did not get it.* He "satisfied his own mind," and that was enough! But alas, for this hasty conclusion! the chemical testimony for the defence most completely demonstrated that the colour-test (the orange-red precipitate), with all its subsequent reactions, save in one single point, *might be exactly imitated without the presence of an atom of antimony*, as we shall presently see.

Again, the witness was not thoroughly clear as to the purity of his reagents; thus, "he had not tested his sulphuric acid, within at least the space of six months." He spoke of "detecting impurities in strong mineral acids by sulphide of ammonium,"—an unheard-of method, and one chemically impossible. He affirmed that "it was impossible to extract tartar emetic, as such, from the stomach of a dead person; nobody had

ever done it; let it once enter into solution, and it is beyond the reach of mere mechanical separation." Why, surely he overlooks the beautiful and simple process of *dialysis*, by which crystalline bodies (including, of course, tartar emetic) can easily be separated from the colloids.

Prof. Aikin testified that he had not been informed what medicines had been administered to Gen. Ketchum; also, that he did not know what were the contents of the stomach at the time of his death. He "was unable to say what would have been the result of his analysis as to colours, if Gen. K. had taken chloral and yellow jessamine."

Moreover, "he had kept no laboratory notes," but only some very scanty memoranda of his analysis made early in July, which he subsequently destroyed; but he "trusted to his memory" in making out his Report, about the middle of October.

In analyzing the contents of the tumbler, Prof. Aikin adopted the same process that he had employed in the examination of the stomach, using, however, *only one grain* of the material. He "did not test all portions of the sediment, to see that they were the same," nor did he test "the organic matter of the sediment." In neither of his analyses did he deem it of any importance to isolate the *metal*, which he said he could have done by Marsh's process; but which, extraordinary as it may seem, he neglected to do. We may remark, that, besides Marsh's process, chemists are acquainted with four other methods of reducing the metal antimony, viz.: Reinsch's process, the galvanic process, the precipitation on tinfoil, and the blowpipe.

It will be remembered that, after the cross-examination of Prof. Aikin, and the failure of the prosecution to sustain the allegation of poisoning by the chemical analysis, on the fifteenth day of the trial, the State produced a new analyst—Prof. Tonry, of the Maryland Institute, Baltimore, who had been employed, *without the knowledge of the defence*, to make a new chemical examination of the liver and other viscera of Gen. Ketchum's body, which was the second time disinterred for this purpose, by Drs. Williams, Miles, and Chew, by order of the State. Mr. Tonry stated that his analysis was only partially completed; and the court granted him several days to finish it. This action was energetically opposed by the counsel for the defence, as unfair and unprecedented; but their opposition was overruled by the court.

This witness testified that he was told by Drs. Williams and Chew, "to examine only for antimony." He described his method of analysis with the most elaborate detail and extra caution, although he admitted that his drying-tube (chloride of calcium) had been used before, and therefore might have contained impurities. He exhibited, as the result of his experiment with Marsh's apparatus, several minute dark spots, on a porcelain lid, which he took to be either arsenic or antimony. There were, in all, thirteen of these minute spots, each about the size of a small pin's

head. Prof. Tonry stated that "he supposed he could have gotten more of them." In order to determine whether they consisted of arsenic or antimony, he applied "to one or two of them, a drop of sulphide of ammonium; the greater part dissolved readily." This he believed was antimony. "The more difficult solubility of the remaining speck, or part of the spot, would denote arsenic." He failed to obtain any evidence of *metallic* antimony by fusing the precipitate from sulphuretted hydrogen, when mixed with cyanide of potassium and carbonate of soda, on charcoal; although "he obtained the metal" by a comparative test, by treating "a very small quantity of tartar emetic with the same flux." But "he was led to believe that the quantity of material that he had first operated upon was too small for the production of the metal."

He next proceeded to estimate the quantity of the assumed antimony, as follows: A portion of the original acid solution was treated with chlorate of potassa, "to see if he could get rid of the colour;" that, he precipitated with sulphuretted hydrogen; washed the precipitate; dried, and dissolved it in strong muriatic acid; diluted with water, and again treated it with sulphuretted hydrogen. "The precipitate thus obtained was not of the bright colour of that from tartar emetic, but it seemed to be modified by some colouring principle, which he could not get rid of." Still, this "did not cause any doubt in his mind that it was antimony."

He then stated that he weighed the dried precipitate, and "the weight was $\frac{2.5}{100}$ths of a milligramme; this would be equivalent to four milligrammes of the sulphide for the whole solution, or $\frac{6}{10}$ths of a grain."[1] From this, he estimated that "there was a total amount of antimony, in the liver, of half a grain." Let us examine this result. First, it was a grave error to assume that the dried precipitate consisted exclusively of sulphide of antimony. It most certainly contained a mixture of organic matter and sulphur—the latter in very considerable quantity, since he exposed his precipitate, from Wednesday to Friday, to the air. There might have been here the merest trace of a metal, or there might have been none at all. Prof. Tonry's idea that "his precipitate could not contain sulphur, because he washed his sulphuretted hydrogen," is remarkable for its novelty, as well as for its impossibility. He testified that "in the solution, there would be an excess of sulphur, but *not* in the precipitate"!

Secondly, his mode of weighing was not accurate. He first dried his precipitate in the beaker "on a water-bath." This would not deprive it completely of adhering moisture. Again, he only weighed it once: it

[1] Mr. Tonry is somewhat confused in his figures: four milligrammes (0.004 grm.) are about equal to $\frac{6}{100}$ths of a grain, and not $\frac{6}{10}$ths of a grain, as stated in the text. In his cross-examination, he gave 2.5 milligrammes as the weight of his precipitate; this would give about $\frac{6}{10}$ths of a grain as the weight of the precipitate from the whole solution.

should have been weighed repeatedly, until it ceased to lose weight by drying. Due care was not exercised in transferring the beaker to the balance, to protect it from moisture, or dirt from the hand. All the above elements of error very seriously affect chemical results, when the question is about fractions of a milligramme; the very errors may account for all the alleged results, in such delicate manipulations.

Mr. Tonry made another examination of portions of the kidneys, spleen, and pancreas, during the interval allowed him by the court, employing the same method as before. By Marsh's process, he obtained five very minute spots of a dark colour, which, he said, "were not very volatile by heat, and readily soluble in sulphide of ammonium." These spots "he had no doubt were either antimony, or arsenic," *but he did not prove them.* The witness also testified that he had obtained "a ring on the delivery-tube of Marsh's apparatus, by putting a spirit-lamp under it;" but for some unknown cause, he did not produce this ring in court, so that we are left in much doubt concerning it. He further stated, that he treated a portion of the original acid solution with sulphuretted hydrogen, "which gave a black precipitate; that, when this was treated with strong muriatic acid, it was dissolved;" then, on passing sulphuretted hydrogen through this acid solution, he "obtained the unmistakable colour of antimony, more so at first than afterwards." We have already alluded to the fallacy of this test, under the circumstances; and we must insist that the experimenter was, consequently, not authorized to conclude that antimony was present.

Besides Professors Aikin and Tonry, the State also called Dr. J. H. Thompson, Professor of Physiology of Georgetown College, and Dr. Benj. F. Craig, Analytical Chemist, of Washington. The former testified to having seen a fatal case of tartar-emetic poisoning at St. Bartholomew's Hospital, London, in 1847, in which the symptoms were "vomiting, purging, a burning sensation of the stomach, giddiness and excessive prostration." The person recovered from that dose, but some hours after, took another, after which "there was but little vomiting and purging, pulse rapid and very feeble, skin cold and clammy, face of a dusky hue, and death in violent tetanic spasms." The witness thought that, from the hypothetical case presented, the party "did not die from natural causes," although "some of the symptoms would correspond with a number of diseases."

Dr. Craig testified simply to the written report of Prof. Aikin, "that his tests were the usual tests to discover the presence of antimony, and were in his opinion sufficient. The obtaining of the red colour with sulphuretted hydrogen, under the circumstances, and the obtaining of a white precipitate by the action of water on the acid solution of that red precipitate, are sufficient to make the presence of antimony certain. All other substances known to science, except antimony, would have given a different colour."

This witness evidently was not acquainted with the action of sulphuretted hydrogen on acid solutions of gelsemium and other organic sub-

stances, and the subsequent behaviour of the precipitate with muriatic acid and water.

The chemical experts summoned by the defence were Professor R. S. McCulloch, of Washington-Lee University, Lexington, Va.; Dr. F. A. Genth, Analytical Chemist, of Philadelphia; Dr. John J. Reese, Professor of Medical Jurisprudence and Toxicology in the University of Pennsylvania; and Prof. H. C. White, of St. John's College, Annapolis.

The testimony of these witnesses was, in the main, very similar. They all agreed that in every case of metallic poisoning, where the issues of life and death are involved, it is absolutely essential to obtain the *metal* in sufficient quantity, and bring it into court, so as to allow the experts for the defence to operate upon it, and prove the poison. Another point insisted upon by them was the absolute necessity of excluding all extraneous matters—such as sulphur and organic matter—in the precipitate, which is to be weighed for the purpose of estimating the actual amount of the discovered poison. Now, in the present case, as has been shown, this most obvious point was totally neglected, and the attempt was made to cover it up, by flatly denying the necessity of the precaution!

A very remarkable and interesting portion of the testimony of these witnesses was that which had reference to the similarity in appearance, between the precipitated sulphide of antimony and the precipitate obtained by passing sulphuretted hydrogen through just such a mixture as was found in the stomach of Gen. Ketchum, after treating it with hydrochloric acid. The colour is, in both cases, orange-red. We have already adverted to the fact, known to all chemists, that when sulphuretted hydrogen is passed for some time through an acid solution containing organic matter, a precipitate, more or less copious, will be obtained, of a yellowish-brown colour; it consists merely of precipitated sulphur and organic matter, and it might be mistaken for an impure precipitate of either arsenic or antimony. It remained, however, for Professor McCulloch to develop the interesting and important fact, that, if the organic substance in the solution acted upon by the sulphuretted hydrogen be the tincture of yellow jessamine and chloral (the latter is not essential), and sufficient time be allowed—say several hours—the colour of the precipitate is of a decided *orange-red*. This effect may be speedily brought about by dropping sulphide of ammonium into a slightly acidulated solution of the jessamine-tincture and chloral. The colour of the precipitate here very soon becomes a rich orange-red, which would defy any chemist to distinguish from the (supposed) characteristic orange-red sulphide of antimony.

Of course, as is well understood by the chemist, the only point of identity is the *colour* of the precipitates. It is true that the colour of the organic precipitate soon undergoes a change, becoming gradually darker, and finally almost black. But the fact of real importance and interest to the toxicologist is, that there are other matters beside antimony which will give the

"characteristic orange-red" precipitate; and that to rely upon this colour-test alone, in the search for antimony, might be attended with the most serious and fatal mistake. In the present case, for example, there can be no doubt that the contents of Gen. Ketchum's stomach comprised the gelsemium, chloral, and milk which he took just before his death. Now, inasmuch as Dr. Aikin's mode of analysis did not get rid of this organic matter, and as we have just shown what would be its reaction with sulphuretted hydrogen, we ask what proof can be adduced of the presence of any antimony, judged by this particular test?

But, it will be alleged that, in testing for antimony, the orange-red precipitate must be subjected to farther proofs, viz.: its solution in boiling hydrochloric acid; the giving a copious white precipitate when this solution is thrown into water; the solubility of this white precipitate in tartaric acid; and the change of colour in this same precipitate into orange-red when touched with sulphide of ammonium. Now, how far will the organic precipitate from gelsemium, chloral, &c., follow out the above particulars? We reply, that the singular analogy holds good in every particular except one—the solubility of the white precipitate (or cloud, according to Prof. Aikin) in tartaric acid. Thus, the orange-red organic precipitate is, at least partially, soluble in boiling hydrochloric acid; this solution when thrown into water gives a white cloud, which ultimately becomes a precipitate; and this precipitate, if still moist with the acid solution, becomes reddish when touched by sulphide of ammonium. We reaffirm here, what we stated in our evidence, that this discovery of Prof. McCulloch has opened a new and important field of chemical investigation, in relation to this particular line of testing for mineral poisons. Surely, if *three* of the links of this chain of "characteristic" proofs of the presence of antimony be broken, and the only other one left is possibly faulty, the chemist can repose very little confidence in its support. He must look to other less defective tests; in fact, in medico-legal cases, he should be content with nothing short of the production of the metal (in metallic poisons), and that in sufficient quantity to be subjected to *all* the known reactions.

In relation to the dark coloured "spots" exhibited by Prof. Tonry, we have seen that there was a failure, in the first place, to identify them as either antimonial, or arsenical deposits. Their extreme minuteness could not have permitted him to manipulate with any certainty as to the result. By an actual comparison of these spots with some trial spots made by Drs. Genth and Reese, it was estimated that each of Mr. Tonry's spots represented only the one-twelve-thousandth to the one-twenty-four-thousandth of a grain of tartar emetic (supposing the spot to be antimony). The whole thirteen stains would therefore only represent about the one-thousandth to the one-two-thousandth of a grain—a quantity so very minute (even if it be admitted to be antimonial, or arsenical) that it might readily be accounted for, by its accidental introduction through the reagents, or

otherwise. Dr. Taylor and other high authorities distinctly state that it is unsafe to rely upon results which are obtained by operating on quantities less than the 150th to the 200th of a grain. And yet Mr. Tonry says that "he partially dissolved one or perhaps two of these minute spots in sulphide of ammonium," and came to the conclusion that it was composed partly of antimony and partly of arsenic! "On one or two other of the spots he tried nitric acid, to see if he would get the white oxide of antimony," but "he got no satisfactory results."

Dr. Genth and Profs. Reese and White testified that they deemed it impossible to manipulate upon such minute spots, with any degree of certainty. The former witness asserted that he had operated upon eight or ten of the antimonial spots made by himself (each of which was at least double the size of Mr. Tonry's) with sulphide of ammonium, but he could not obtain a reliable result. In the celebrated Laffarge case, the 130th of a grain of arsenic only was found, and that after an enormous quantity of chemicals had been used up. Dr. Taylor alludes to the chemical evidence in this case as "of a most unsatisfactory kind," and such as "should have been rejected by the court." "No man," says he, "with any respect for his character, or for the common sense of a jury, would base chemical evidence on the thousandth, or less than the thousandth part of a grain of poison, in a case of life and death."

There is another inaccuracy in Prof. Tonry's experiments. He stated that "he added potash to dissolve the (supposed) sulphide; he presumed it would form antimoniate of potash; he then added tartaric acid with the object of obtaining tartar emetic." This is manifestly a chemical impossibility, since, as every chemist knows, the reaction of potassa with the sulphide of antimony results in a partial double decomposition, giving rise to the oxide of antimony and the sulphide of potassium; and further, on the addition of an acid, a precipitate of the oxy-sulphide of antimony would be thrown down. Since this did *not* occur with Mr. Tonry, the inference seems unavoidable, that he could not have been operating with an antimonial compound.

Finally, in regard to Mr. Tonry's delicate *weighings*. He testified that "his balance was guaranteed to weigh the twentieth of a milligramme" (about the $\frac{1}{1300}$th of a grain); and that, on it "he could weigh the three-thousandth part of a gramme" (about the $\frac{1}{300}$th of a grain). Prof. White testified that he had been Mr. Tonry's predecessor in the Maryland Institute; that he had fitted up the laboratory there; that "he did not think much reliance could be placed on delicate weighings made there, because the building is shaky, being built on made ground." Further, that whilst the weighings there might be sufficiently accurate for commercial purposes, he himself "would not like to weigh there less than the $\frac{1}{60}$th to $\frac{1}{70}$th of a grain."

The careful analysis of all the scientific testimony leads us to the fol-

lowing conclusions: Dr. Williams, the physician of Gen. Ketchum, who certainly had the best opportunity for observing, had no suspicion of his patient being poisoned, throughout his whole sickness, until about an hour before his death. The idea then seems to have entered his mind in consequence of his being shown a tumbler containing a suspicious looking sediment, that had been found in a distant part of the house. The doctor's suspicions, very naturally, were "strengthened into convictions," when he learned, in a few days, of the alleged discovery by Prof. Aikin, of tartar emetic, both in the stomach of the deceased and in the sediment of the tumbler. It would seem, perhaps, only natural that Dr. Aikin's colleagues in the University—Profs. Chew, Miles, Donaldson, Howard, Smith, and Johnston—all gentlemen of high character and standing in the profession, should be influenced by this fact, in the testimony which they gave at the trial, as to whether the death of Gen. Ketchum was to be attributed to a natural or a non-natural cause.

When, however, the chemical analysis came to be sifted, it was found to be altogether inconclusive; there was no proof of the presence of antimony in the body of the deceased; the tests employed were shown to be fallacious; and in fact, the whole chemical evidence completely broke down. Notwithstanding this disproval, so powerful had been the mental impression of the presence of poison, as the main element connected with Gen. Ketchum's death, that the witnesses could hardly disabuse themselves of it. All we desire to insist upon here is the very strong presumption that the medical testimony of the State was materially influenced by the *assumption* of the actual discovery of tartar emetic in the body of Gen. Ketchum, by the chemical analysis.

As regards the medical testimony for the defence, the position assumed, and maintained with considerable ability, was that the death of Gen. K. could be accounted for most logically, and most consistently with sound physiological principles, on the theory of *disease*, and not on the supposition of tartar-emetic poisoning. To support their position, they appealed both to the symptoms before death, and to the post-mortem signs. They argued that while the former were in some particulars not absolutely inconsistent with the symptoms of tartar-emetic poisoning (inasmuch as there are no symptoms exclusively characteristic of this poison), they were far more in accordance with certain diseases affecting the great nervous centres, and especially with cerebro-spinal meningitis. Some of these witnesses asserted the absolute identity between the symptoms of the deceased and those of cerebro-spinal meningitis—especially the fulminant form. Others, again, without being quite so positive, testified to the strong probability of this disease having been the cause of death. Several of the Baltimore physicians deposed that cerebro-spinal meningitis was very prevalent in that city, at the time of Gen. Ketchum's sickness, though this was denied by others. It was admitted also by all the medical experts for the defence,

as well as by some of those for the State, that diseases do sometimes prove fatal, without leaving any discoverable lesions. Numerous standard authorities were cited to support this fact, in relation to the fulminant form of cerebro-spinal meningitis.

Scores of professional men, of the very highest reputation in the country, have coincided with the opinions above expressed; among whom we will only mention here Dr. Fordyce Barker, of New York, who has written a letter, quoted in the *N. Y. Herald*, strongly endorsing this view of Gen. Ketchum's case. We will here briefly quote from an authority of acknowledged reputation, in relation to the value of *symptoms* exclusively, as affording an indication of poison: Christison says, "It is now laid down by every esteemed author in medical jurisprudence that the symptoms, however exquisitely developed, can never justify an opinion in favour of more than high probability." And again, when treating of arsenic, "The present doctrine of toxicologists and medical jurists seems universally to be, that symptoms alone can never supply decisive proof of its administration." "All these symptoms may be caused by natural disease;" and "consequently, every sound medical jurist will join in condemning unreservedly the practice which prevailed last century, of deciding questions of poisoning in such circumstances, from symptoms alone."

The only remark that we would offer here in connection with the chemical testimony, has reference to the remarkable analogy between the results of the action of sulphuretted hydrogen on antimony, and on certain organic bodies, as explained above. We deem it highly probable that other organic substances besides gelsemium and chloral, will be found, which will render this analogy even still more complete. Certain it is, that the researches so far have settled the question of the inadmissibility of what has, heretofore, been regarded as very strong proof of the presence of antimony, in toxicological investigations; and they still further strengthen the position taken by the defence, of the absolute necessity of "producing the metal" in all criminal cases of metallic poisoning.

Before taking leave of this subject, we cannot forbear adverting to one or two incidental points. The first is, the impropriety of conducting a post-mortem examination, where there is a suspicion of death by poison, and where the reputation and even the life of a fellow-being is involved, without the presence and oversight of the legally constituted authority—the coroner. It must be evident to every observer, that the whole examination, in the present case, was in the hands of those who were not entirely devoid of prejudice. The first autopsy is made by Drs. Williams, Miles, and Chew, within twenty hours after death—the strong suspicion of poison influencing the minds of these gentlemen. Two weeks later, the body is exhumed and is again examined, exclusively by these same persons. There is no representative or friend of the accused party invited to be present. Surely, this was not in accordance with justice or propriety. Were it not that the

professional and social character of the parties concerned places them above suspicion, there might certainly be very grave reason for taking exception to such *ex parte* proceedings; for there might be just cause for doubt, in relation to the absolute identity of the material operated upon. But still more glaring was the departure from propriety, when the body was the *second time* disinterred, and that too during the actual progress of the trial. The State, finding its case completely failing by the breaking down of the evidence of Prof. Aikin, secretly dispatched Drs. Williams, Chew, and Miles to Washington, with instructions to again exhume Gen. Ketchum's body, and remove the liver and other viscera, for a second chemical analysis. We say this thing was done *secretly*, for no friend or representative of the defence was apprised of it. Possibly, one or more of the judges knew it. Certainly, none of the counsel for the defence was aware of it, until Dr. Williams, one morning, gave the unexpected detail to the court, and informed them that the new chemist, to whom the analysis of the viscera had been committed, was now ready to give a partial exhibition of his results!

Now, it seems to us that this procedure on the part of the State was unfair to the accused, to her counsel, and to the cause of justice. We deem it contrary to all precedents in criminal jurisprudence, to take such an unfair advantage of the accused. Why was not her counsel informed of this intention of the prosecution, so that at least one of the experts for the defence might have been present at the exhumation of the body and the subsequent analysis, and thus give at least the appearance of fairness to the procedure? We venture to assert, without fear of contradiction, that such an *ex parte* proceeding would never have been permitted in any country in Europe; and will never again be allowed, it is to be hoped, in our own country.

One other point only we especially regret to be obliged to animadvert upon, and that is the unnecessary harshness exhibited by the learned attorney-general towards some of the witnesses. That even distinguished lawyers should be proficients in all the mysteries of chemistry or medicine, is not to be expected; but when such undertake to cross-examine scientific experts upon their own specialties, and perplex and embarrass them by a bungling mode of putting questions, which it is simply impossible for the witness to answer, we can hardly allow the ignorance of the questioner to be a sufficient excuse for a course of procedure such as we would expect to find only in a police court.

It strikes us also as being in exceedingly bad taste, and as by no means strengthening his cause, for the law officer of the State to make such an assertion as the one made by the attorney-general, "that the experiment and testimony of Professor McCulloch were either a fraud upon the administration of justice, or were the result of the grossest ignorance." Inasmuch as Professor McCulloch's experiments were repeated and verified by

Dr. Genth, and by Professors Reese and White, the attorney-general's vituperation was a virtual arraignment of all the chemical experts for the defence. It is scarcely necessary to reply, after what has been noted in connection with the chemical testimony for the prosecution, that the above remark was not justified by the fact that Professor Aikin failed to substantiate the experiments of Professor McCulloch; since it is sufficiently manifest from this trial, that the former chemist obtained results which the others could not verify; whilst, on the other hand, he was not able to verify the results which all the others testified that they had obtained.

We have thus endeavoured to give an impartial review of the scientific testimony connected with this trial; adverting also to certain important points incident to this evidence. With the *circumstantial* evidence, we, of course, have here nothing to do; nor is it our province, on this occasion, to express an opinion either as to the guilt or innocence of the accused party. As is generally the case in such trials, there was a considerable conflict of professional opinions, both medical and chemical, among the experts; and since it might be deemed invidious for us to attempt to decide for either party, we must be content to leave the decision with the intelligent reader.

PHILADELPHIA, 1840 Green Street.

ART. III.—*Note respecting the First recorded Case of Astigmatism in this Country for which Cylindrical Glasses were made.* By HENRY D. NOYES, M.D., of New York.

BY the kindness of Dr. Gray, of the New York State Lunatic Asylum, Utica, I have come in possession of a pair of cylindrical glasses, which I suppose were the first ever made in this country. They belonged to Rev. Mr. Goodrich, who at the time of his death was chaplain to the asylum. They were given to me wrapped in a piece of writing paper on which, in the owner's handwriting, was inscribed the following memorandum:—

"No. 7 French No. (number) cylindr. con., got of McAllister, May, 1828. I wore 14 months, *i. e.* till July, 1829. Again put into use March 11th, 1843; they were laid aside Dec. 10th, 1850."

Why they were laid aside I do not know, the owner at the time of his death was of considerable age, and at 1850 was about 50 years old. It seems that they could not have perfectly met the necessities of his vision, because he wore them only for certain periods of time. But these were long enough to show that from them he derived great advantage. The glasses, I find, are plano-concave cylindrics of 7 inches focus, with axes horizontal, and were mounted in a spectacle frame with oval eyes. The maker is the old and well-known optician, Mr. John McAllister, of

Philadelphia. These glasses have become historical, as I have no doubt they are those to which reference is made in the American edition of *Lawrence on the Eye*, page 669 (1854), in a foot note by Dr. Hays. In that statement the number and kind of glass is not given, and in the allusion to it, which Donders makes, in his work on refraction and accommodation, page 541, New Sydenham Society's ed., 1864, he calls attention to this circumstance. The account of the matter as given by Dr. Hays, in Lawrence, is in the words of the clerical gentleman who used the glasses, and perhaps it will be interesting to refer to it to learn how the choice of these glasses was arrived at.

"From my earliest years I have been sensible of a deficiency in sight, which I was induced to call near-sightedness, because I was obliged to approach nearer to objects to see them, than most persons, which is still the case. This deficiency, until I was about 16 years of age, was accompanied with weakness of sight, but my eyes are now strong. I commenced my studies regularly at the age of 19, and am now 24. Close study has had no other effect on my sight than to strengthen its endurance, but not its precision or length. My right eye is and always has been much better than my left, but the defect in both is precisely of the same nature. At the age of 16 I procured a pair of plain green glasses, which were some little assistance in moderating the intensity of the light. I tried to get concave glasses, but could find none that were of any essential assistance. I afterwards used a small convex lens of about 10 inches focal distance, as a microscope, to assist me in reading, which was some assistance, but during the five years of studying, I have used no glasses whatever. In November last (1825) I procured a pair of concave glasses in New York, about 5 or 6, which will accompany this letter. Until then I had never discovered that a change of position had any influence on my sight; but in looking through these glasses I found that any object whose length is in a horizontal direction appears much more distinct than a similar object whose length is perpendicular to the plane of the horizon. Thus the cross-rigging of a ship appears much more distinct than the mast and perpendicular ropes. I have since been led to make experiments first on my naked eye, and then on it in connection with my glass which I send you, and the result at which I have arrived has been universally the same.

"Suppose I stand in an erect posture, then let $f\,h$, $e\,g$ (Fig. 1) be any object, the parts of which are equal and similar, and of which $f\,h$ is perpendicular to, and $e\,g$ parallel to the horizon. Then the part $f\,h$ will appear plain, but the part $e\,g$ will not. Now, if I turn my head from a perpendicular to a horizontal position, the reverse takes place, that is, $e\,g$ appears plain, but $f\,h$ does not.

Fig. 1.

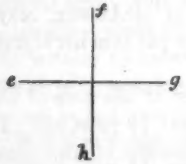


Fig. 2.

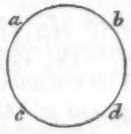
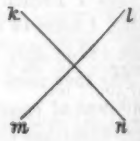


Fig. 3.



"Again, let $a\,b\,c\,d$ (Fig. 2) be a circle; then if the above statement be correct, when I stand erect the parts $a\,c$ and $b\,d$ will appear plain, but $a\,b$, $c\,d$ will not, which is the fact. Furthermore, if I turn my head a right angle (that is to a horizontal position) then $a\,b$, $c\,d$ become plain, but $a\,c$, $b\,d$ do not. It seems almost superfluous to add that a figure like $k\,l\,m\,n$ (Fig. 3) appears uniform in both the lines $k\,n$ and $l\,m$, when the body is in an erect posture.

"These facts would lead me to conclude that the crystalline lens of my eye is cylindrically convex or, perhaps, oblately convex, and that its greatest length is in a perpendicular direction (that is, in a line parallel with the length of my body); consequently I would conclude that a glass whose shape resembled the probable shape of the crystalline lens of my eye, placed before my eye so that its greatest length would be at right angles with the lens of the eye, would produce a perfect image.

"All that I have said in relation to the diagrams above is strictly true in relation to my naked eye, and if I could here wind up the difficulty I should stand in some hope; but the fact is the moment I put on my concave glasses everything is exactly reversed; the line *f h*, which before appeared more distinct than line *e g*, now becomes as much less distinct, and in relation to the circle the parts *a c* and *b d*, which were before more distinct than the parts *a b*, *c d*, now become as much less so.

"The figure *k l m n* will appear much as it did to the naked eye, except that it is a little more distinct, and, perhaps, a little smaller; but what I mean is that it appears uniform through the glasses as it did to the naked eye. If it be true that my eye requires a glass whose shape does not correspond to a portion of a true sphere, but rather to a portion of a spheroid or, perhaps, a cylindric, I cannot tell from any observation I have yet made on my eyes, whether the glasses should be of the concave or convex form. The great difficulty is why should the appearance of objects be entirely changed by the interposition of a concave glass? When I purchased my glasses I was not aware of this curious fact in regard to my eyes either with or without glasses. I only knew that I got those through which, on the whole, I could see best. If it be asked whether I consider the glasses in question are essential assistance to my sight, I answer that if my object be to examine a horizontal object, they truly are assistance, and they are so in the examination of a perpendicular one, provided I turn my head a right angle."—*Lawrence on the Eye*, Amer. ed., edited by I. Hays, M.D., 1854, p. 669.

His observation of his symptoms was able and acute, and his conjecture as to their probable cause correct in an optical sense. He doubtless had simple astigmatism, or the spherical glass needed in addition to the cylindric must have been weak.

In reviewing the above case, it may be instructive to note some of the points which the writer with keen observation makes prominent.

1 He used a convex 10 glass in his youth as a magnifier, with some advantage. By enlarging the image the indistinctness of outline became less conspicuous, although the actual definition was not improved. This use of a convex spherical glass where the error called for a concave cylindrical glass is paralleled by some cases of hypermetropic persons who, to help their vision at a distance, have resorted to concave instead of convex glasses, and by extraordinary taxation of accommodation with reduction in the size of images, have been able to see what otherwise they could not.

2. The illustration from the rigging of a ship is very apt, and is worth remembering in questioning patients. It is evident that the vertical meridian was the strongest, and the horizontal must have been almost normal. With his concave spherical glasses on, horizontal lines were distinct and vertical ones indistinct; without his glasses the reverse was true, as he illustrates by his figures. Supposing his eyes to be myopic ¼ in the ver-

tical meridian, the mast of a vessel would appear on its sides distinct, but at its ends be very hazy, because the rays entering the eye in the horizontal plane come to a correct focus, while the vertical ones meet in front of the retina; but this error cannot show itself in the image of a vertical line except in its extremities. On the other hand, horizontal elongated objects, like the spars of a vessel, would appear clean cut on the ends, the only portion where the correct refraction of the horizontal meridian of the eye could show its effects, but its margins, in virtue of the fault in refraction in the vertical plane, would be unclear.

If the author had ever happened to resort to the simple device of looking between his nearly-closed fingers, extended before him horizontally, making thus a horizontal slit, he would have been pleased and amazed to find that both spars and masts would be well defined, although less bright.

3. For lines oblique to the horizon there could no distinct image, and, therefore, figure 3 appears alike in both limbs. He failed to note, in looking at a circle, that, besides the distinctness in definition of its sides as compared with its top and bottom, its shape became ovoidal with the long axis vertical.

4. The supposition that his lens must be cylindrically or oblately convex with its long axis vertical, was founded upon a wrong explanation of the reason for the greater distinctness of vertical lines. This superiority being in fact due to the good definition of the sides of such lines, which results from the correctness of refraction in the horizontal plane. Vertical lines may be assumed to be made up of a succession of square dots, or circles in close contact. Now, as the sides of circles appear clear, the addition of many dots to each other in a vertical sense gives a line whose sides are clear but ends dim.

5. By putting on concave spherical glasses, which carry the focus farther back for all meridians, the vertical meridian becomes correct, and causes the sense of clear definition to be just the opposite of what it was, viz., for horizontal objects, because top and bottom are clear, while the ends run out in brushes. But the horizontal meridian of the eye is now overcorrected and made hypermetropic, the focus going behind the retina. This error can in some measure be overcome by vigorous accommodation, and the form of a square object might be tolerably well made out by permitting the eye to rest without effort on its horizontal boundaries, and by making a strong accommodative effort in scanning its vertical contour. In this way a person with simple myopic astigmatism may gain clear sight with concave spherical glasses.

The naive suggestion, that, by turning his head a right angle in viewing successive parts of an object, he could compass correct vision, was rendered superfluous by the cylindrical glasses. The case is remarkable, and one reflecting credit upon the clerical patient and upon the optician. Physio-

logical optics had not yet entered upon the high career to which it has since been brought, and what in 1826 was only dimly understood, was in 1856 by Helmholz, and in 1864 by Donders, brought to the light of perfect demonstration.

NEW YORK, 73 Madison Avenue.

ART. IV.—*Observations on the Eustachian Tubes, with Description of an Instrument for their penetration by Rhinoscopy.* By EPHRAIM CUTTER, M.D., of Woburn, near Boston, Mass. (With four wood-cuts.)

ACCORDING to Tröltsch most anatomists suppose the Eustachian tubes to be the remains of the first embryonal bronchial fissure, probably because they are compound structures, being composed of membrane, cartilage, and bone. The average length (Tröltsch) is about $1\frac{1}{2}$ inches, or, more accurately, 35 millimeters, of which 24 belong to the cartilaginous and 11 to the osseous portion.

At the pharyngeal orifice it is 9 mm. in height, in breadth 5 mm. At the tympanic orifice it is 5 mm. in height, in breadth 3 mm. At the inner end of the cartilaginous portion it is 2 mm. high, and 1 mm. wide. Thus the Eustachian tube is not uniformly cylindrical, but slightly trumpet-shaped at both ends, or rather two frustrums meeting at their narrow extremities; *vide* Fig. 1, which represents an outline of the tube according to the above measurement of Tröltsch. An Eustachian tube which the writer removed from a dissecting-room subject, aged eighty years, presented a uniformly cylindrical appearance, much different from that stated by Tröltsch.

Fig. 1.

The constituent parts are not diffused equally throughout the whole calibre. The cartilage composes about two-thirds of the tube which lies toward the pharyngeal orifice. This two-thirds is disposed on the posterior part of the tube. It is a thin lamina, difficult to separate by dissection. The remaining third of the tube is membranous. This disposition of cartilage is analogous to that in the trachea. The evident object is to secure a patency of the tube.

The membrane of the inside is corrugated in the direction of the long axis of the tube. It is covered with ciliated epithelium.

The position of the faucial end of the Eustachian tube is described to be opposite the middle turbinated bone. When it is known that in some cases a rhinoscopic examination shows *an entire absence of the middle turbinated bone*, the accuracy of this statement may well be questioned. The fact is that the Eustachian orifice opens at different points in different subjects. Sometimes, as in the writer's case, it is found to be

opposite the upper and outer quarter of the posterior nasal openings. Sometimes it is opposite the middle, and sometimes opposite the lower quarter. Thus Fig. 2 shows this variety in position.

Fig. 2.

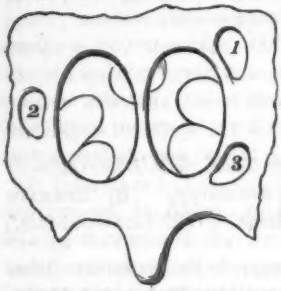


Diagram of the Posterior Nares, looking from within outwards: 1. Is the position of the Eustachian orifice in the writer's case; 2. Is the second position named; 3. Is last position. *Vide* Mackenzie on the Laryngoscope, edited by Dr. J. Solis Cohen, Philada. ed., 1869, p. 282.

With this variety of position how can rules given for the introduction of the Eustachian catheter through the nose be definite? Why not call in the aid of the rhinoscope, and ascertain the situation of the orifice before attempting a catheterization? Otherwise it is guess-work; an inexact procedure, unworthy of the present state of our knowledge.

The direction of the Eustachian tubes is generally described as from within outwards, backwards, and upwards. In order to give more definiteness to this description, the writer measured the angle which the Eustachian tube makes with the antero-posterior vertical median plane of the head. One of the cases was in a specimen in the Warren Anatomical Museum, from a child eighteen months of age, access to which was kindly given by Prof. Jackson, curator. A straight probe was thrust into the Eustachian tube and held there by contact. A large portion of the probe projected. The half head was then held over a straight line marked on a piece of paper in such a manner as that the vertical surface corresponded with the line. The points where the projecting probe covered the line, and where it terminated over the paper were marked. On connecting these two points with each other by a pencil-mark, the angle of obliquity was given. This was subsequently measured on a tractometer (an instrument for measuring angles), and was found to be 43° 30′. *Vide* Fig. 3.

Fig. 3.

C D represents the antero-posterior median line of a vertical section of the head. *A B* is the probe marking the direction of the Eustachian tube. The angle represents the obliquity of the tube.

The other case of admeasurement was in a dissecting-room subject, female, 82 years of age, through the kindness of Dr. John W. Mead, of Lowell. The obliquity was the same as in the first case. So that probably one would be safe in saying that the direction of the Eustachian tubes is in the neighbourhood of 45° backwards. In the cases measured there was no upward direction as the probe stood horizontally. The writer wishes that observers would measure this angle on different subjects as they have opportunity, and report the same, even if it is only

one observation. It is only by multiplied scrutinizations that general laws can be laid down. A report must not be despised because it involves only a *single* instance, because experience is but an aggregation of single events. If the integral parts are withheld there can be no whole.

The question of the patency of the faucial Eustachian orifices is a vexed one. The majority of observers maintain that they are closed *except* during the act of deglutition. We coincide with the view *that they are always open normally*, contrary to what we have before advanced (*Vide Boston Med. and Surg. Journal*, Feb. 1867), "That they are always open except during the act of deglutition."

The following considerations are adduced in support of this position: 1st. The anatomical characters of the tube; 2d. The physiology; 3d. Actual observation.

The *anatomical characters* of the tube we have seen are bony towards the tympanic end; that certainly cannot close, except by being filled up from the inside. This would not be a physiological, but a pathological condition. The remaining portion is partly bony, partly cartilaginous, and partly membranous; so much so that it is impossible to conceive of any contact of the internal walls, except at the pharyngeal orifice. This is more apparent when we consider that the membranous portion of the Eustachian tube occupies only one-third of the circumference. To close up the tube would require the collapse of this membrane, and the bending together of the cartilage; so that the inverted membrane should fill up the space embraced within the concavity of the cartilaginous walls. But the cartilage and the membrane are attached to the surrounding parts, so that the amount of inversion required, which is very considerable, cannot be accomplished. The trachea, which is about two-thirds cartilage and one-third membrane, does not collapse and its walls come in contact during physiological acts. Neither does the meatus auditorius externus, composed of cartilage and bone, ever collapse physiologically. The nostrils are analogous in structure as to bone, membrane, and cartilage, they close only by voluntary act physiologically.

The other orifices in the body that are known to close and open, such as the oral, anal, puncta lachrymalia, urethral, vaginal, œsophageal, stomachic, ileo-cæcal, &c., have anatomical characters not resembling those of the Eustachian tubes. Anatomical structure and analogy point to the function of the Eustachian being always open normally.

The physiology. It is generally believed that the most important of the functions of the Eustachian tube is to maintain a communication between the internal ear and the external atmosphere. Were the internal ear a closed cul-de-sac, the vibrations of the tympanum would be deadened. If the internal ear, besides being a closed cul-de-sac, were a vacuum, the delicate tympanum would be distended inwards, if not ruptured, by the atmospheric pressure of fifteen pounds to the square inch. It seems to

have been the design of the Creator to bury the organs of hearing, which are of exquisite sensibility, deep in a bony cavity, out of the way of harm, and yet so sensitive to the vibrations of the outside air that they are capable of detecting vibrations which play over one thousand times a second!

Now, if the Eustachian tubes did not afford the communication to the outside atmosphere, it is difficult to understand how the function of hearing could be performed at all. Indeed, when the Eustachian tubes are closed, for example by inflammation, its processes, or results, this closure is a sufficient cause for the deafness which results. Now, if the Eustachian tubes are closed *except* during the act of deglutition, would we not expect that deafness would exist except during a meal? Or, if we take the other proposition, one which the writer formerly maintained, namely, that the Eustachian tubes are *closed only during the act of deglutition*, should we not expect a momentary deafness while deglutition was performed?

It was by observing that there was no deafness or occlusion of hearing during the act of swallowing, that the writer has been led to review his position, and to maintain that the Eustachian tubes *are open at all times during normal condition*.

Another function is the affording a canal whereby egress may be found from the internal ear for secretions which form therein, and if retained would mechanically distend and interfere with the function of the internal ear. I doubt whether there are any such secretions *normally;* abnormally they may occur. Whenever they do exist, would they not be discharged better through Eustachian tubes permanently open, than through canals generally shut? So that the function of affording egress favours a patent condition of the tubes.

The Eustachian tubes being at all times open, allows of the introduction of air by diffusion; as in the lungs, the air penetrates the cells at the extremities and sides of the terminal bronchial divisions. The cavity of the tympanum is a closed cul-de-sac. The vibrations of the tympanum and motions of the ossicles cannot exclude all the air, as they are not extensive enough. We are left to fall back upon diffusion as a sufficient and satisfactory cause of the change of air in the cavity.

Take the simple Valsavian experiment, which "consists in pressing the air into the ear, after a powerful inspiration, the mouth and nose being closed." When the organs are healthy the air is pressed and condensed into the cavity of the tympanum, and the tympanum stretched, and is forced outwards, as appears by inspection and subjective sensations. On the other hand, if the mouth and nose are closed, and the air exhausted by sucking and swallowing, the tympanum is stretched inwards. Moreover, if the tympana are absent or perforated, the air rushes out with an audible noise, and water injected into the meatus externus passes freely into the throat. Now, on the supposition that the tubes are closed except

during the act of deglutition, it is difficult to conceive how the air could so easily distend the drums in the Valsavian experiment, or the water pass in the other. If the other supposition, that the tubes are open *except* during deglutition, it is also difficult to reconcile the facts, as during the Valsavian experiment the soft palate is drawn up against the post-pharyngeal wall, crowding, if anything, the Eustachian orifices together towards closure. On the supposition that the tubes are open at all times normally, it is easily seen how air and water can be sucked from or forced and injected through against the air which is in the cavity of the tympanum.

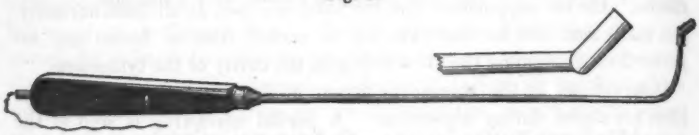
Observations by the rhinoscope favour at first sight the view that the tubes are closed during deglutition. A partial narrowing is seen of the outlet as the soft palate is drawn up by the tensor palati muscles, but as the palate approaches close to the post-pharyngeal wall it obscures the view, so that it is impossible to tell from inspection how the orifices appear when the palate has made its complete air-tight fit against the post-pharyngeal wall. The writer has examined his own and other Eustachian orifices repeatedly, and they always have been found to be open, in what was considered a healthy condition. Dr. L. Elsburg, of New York city, takes the view that the Eustachian tubes are closed by a contact of their inner parietes; that this closure is not tight, but loose. He compares the contact to that of the vaginal walls in healthy females. Air forced easily passes, as the obstruction is so slight. So that practically the tubes are always open. I am obliged to dissent from this view, although its results are the same as mine. On the other hand, in some cases of deafness he has found a closed condition of the orifices. So that on the view that a patency of the Eustachian tubes is their normal condition, it has occurred to him to suggest a means of penetrating them with air or otherwise.

Taking the ground that the Eustachian tubes do not always open into the pharynx at the same points, as shown before, it is deemed advisable, when possible, to make a rhinoscopic examination, and find out where they are placed and what is their condition; then, bearing in mind the direction of the Eustachian tubes, as before described, to approach them from the mouth with a catheter, which is a simple brass or silver tube, eight inches long, three-sixteenths inch in diameter, provided with a handle at one end, and at the other bent up to a right angle for about one inch. This instrument brings one to the *mouth* of the Eustachian tubes. To enter these tubes by air, for instance, another piece of tube similar in size and material, ¼ inch in length, is soldered at a right angle to the long axis of the terminal branch of the tube. At the same time it is placed forwards to the left, if for the right Eustachian tube, at an angle of 43°, with a vertical plane running through the long axis of the instrument. If intended for the left Eustachian tube the terminal bar is placed on the right.

To recapitulate, it will be seen that this instrument follows mechanically

the directions into the Eustachian tubes from the mouth, viz.: 1st. The long axis follows that of the mouth; 2d. The bent portion follows the long axis of that part of the pharynx behind the soft palate; and 3d. The terminal bar follows the angles which the Eustachian tubes make with the pharynx.

Fig. 4.



Eustachian Catheter of the writer.

To use this catheter it is necessary to ascertain the position of the Eustachian orifices by means of the rhinoscope. The catheter, flat side parallel to the tongue, is passed just behind the velum palati. A quarter turn will bring the end uppermost without touching the uvula or palate. Then by means of the reflection in the mirror, the terminal end is brought into contact with, and buried in, the Eustachian orifice desired. All these procedures being done under sight, there can be no question but that the instrument is fairly engaged in the tube. Air or water, or atomized liquids, may then be easily forced into the tube, as deemed advisable by the operator. Air may be blown from the operator's mouth by means of an India-rubber tube, which is slipped over the proximal end of the catheter.

In order to introduce tents into the tubes, a modification of this catheter is necessary. Instead of having the terminal bar soldered on, it is provided with a socket which fits on, and allows of motion in a circle about the end. A small hole is bored through the socket, and a fine wire is passed through and knotted. The other end of the wire is drawn through the tube and brought out beyond and bent up towards the handle. The object of this wire is to hold the socket in place during introduction and rotation. If it is desired to introduce a slippery-elm tent, for instance, the process is as follows: A piece of slippery elm $1\frac{1}{2}$ inch in length is fitted into the cavity of the terminal branch, so that about one inch projects. This portion is rounded, pointed, and moistened with water. It then becomes very slippery. The fine wire is drawn tight and fastened about the handle. If the tent is set at the angle of 43°, it is easily seen that it cannot be introduced, as the point projects too far forward to get safely under the soft palate. It is necessary to set the tent at an angle of 90° with the vertical plane of the instrument. It is then introduced into the pharyngeal cavity, as described; except that just before the point of the tent reaches the soft palate it is depressed behind the tongue until the point clears the velum, and the upward turn is made, which brings it behind and lying parallel to the velum. A sidewise movement brings the

point to the Eustachian orifice, and continued engages it and the tent in the tube—the tent rotating from the angle of 90° to that of 43° as it progresses in the penetration. If desired, the tent and terminal branch and wire may be disengaged and left in. The wire serves as a means of communication, and preventive of swallowing the tent, should it chance to become disengaged and drop out.

There should be one catheter for the right and one for the left.

ART. V.—*On Hectic Fever.* By FRANCIS D. CONDIE, M.D., of Philadelphia.

Is hectic fever to be considered an essential, pathognomonic feature of pulmonary phthisis? Few physicians, we suspect, called to a patient who, after a lingering illness, has become, gradually, greatly emaciated, with a quick and feeble pulse, who is daily subject to attacks of chilliness, more or less marked, followed by flushings of heat, a dry, parched skin, circumscribed redness of both cheeks; one such paroxysm of chilliness occurring in the twenty-four hours, between two and three o'clock of the afternoon, or, not unfrequently two, the second occurring just before night-fall, both paroxysms terminating—the one during the early portion of the night, the other towards morning—in a copious, clammy, and exhausting perspiration; few physicians, we repeat, observing in a patient such a group of symptoms, especially if, in addition, there is short, impeded respiration, frequent cough, with expectoration, would not pronounce him or her to be labouring under tuberculosis of the lungs, and in a large number of cases his diagnosis would be perfectly correct.

The symptoms above described, taken in connection, are unquestionably those most usually observed in patients labouring under pulmonary consumption, especially in the protracted stage of the disease.

Hectic fever, however distinctly developed, is by no means pathognomonic of pulmonary tuberculosis, even when accompanied by impeded or disturbed respiration, cough and expectoration. Such a group of symptoms mark, usually, cases of spurious consumption (chronic bronchitis and pneumonia), while hectic fever is the attendant upon a large number of cases of disease in which no affection of the respiratory organs is present, as is proved by repeated post-mortem examinations. In the course, in fact, of any disease, in exhausted patients in whom there exists a chronic source of irritation, hectic fever is liable to be, is, indeed, generally developed.

Though a common attendant upon pulmonary tubercular phthisis, yet it is not so invariably, and, consequently, is not pathognomonic of that form of disease. According to Louis its presence will be found wanting

in one case out of ten. According to our own experience, however, the fever is absent less frequently than this.

It has been supposed by some that hectic fever is dependent on slow, continued suppuration in some internal part or organ, and by others, that it is caused by the reabsorption of pus from internal or deep-seated abscesses. Such opinions, however, are based on no series of well-established facts. Hectic fever, it is well known to every observing physician, is present in very many cases of pulmonary tuberculosis before any softening of the tubercular deposits has taken place, or any abscess has been formed in the lungs. It is, in fact, in those patients who are of an eminently strumous diathesis, one of the first indications of the mischief going on in the lungs. Hectic fever, we may remark, has, in no case, any direct relation to the extent of the suppuration going on at the time, or which had preceded its onset.

It is a well-established fact that hectic fever is of common occurrence in cases of debility, when the patient has been subjected, for a long time, to some continued source of irritation, but in whom, it is very certain, there is no process of suppuration going on, in any portion of his organism. This, our observations teach us, is especially the case in those who are of a strongly marked nervo-lymphatic temperament. On the other hand, hectic fever is absent in very many cases of chronic disease, throughout the entire course, in which suppuration had been going on for a long time, an impediment existing to the free discharge of the pus. We have had, in numerous instances, under our care cases of paronychia, which had run on to suppuration, and of large collections of pus seated beneath the fascia of the thigh, in cases of neglected periostitis femoris, a disease of not unfrequent occurrence during childhood, and always attended with much suffering and emaciation, but usually without hectic fever.

From the foregoing facts, it will be perceived that, of itself, the presence or absence of hectic fever furnishes no certain evidence of the presence or absence of pulmonary disease, and, under no circumstances, is it of any value in the establishment of a correct diagnosis between the tubercular, and what we have ventured to denominate the spurious form of pulmonary phthisis, it being as often present in the one as in the other.

ART. VI.—*On the Use of Bromide of Potassium in Rattlesnake Bites (Crotulus horridus).* By E. A. ANDERSON, M.D., of Wilmington, N. C.

POISONING by bites of venomous serpents is of frequent occurrence in the Southern States, and particularly in the region in which I reside, and I determined to try the efficacy of bromide of potassium in very large doses

in these cases, being led to do so by the conviction that it was the efficient agent in Bibron's vaunted antidote.

CASE I. On the night of the 24th of April, 1867, I was hastily summoned to Miss C. J., æt. 17, who, while walking in the garden with a young friend, stooped in the dim twilight of a spring evening to pick some violets, when she sprang back declaring that she was bitten in the thumb of the right hand, which caused her extreme pain. This was followed by great prostration, disposition to faint, and coldness of the limbs. At the same time her companion heard the horrid rattle of the snake, and perceived it rapidly gliding away.

I found the patient lying on the sofa suffering from excruciating pain in the thumb bitten, extending up the arm to the shoulder of the same side, great tumefaction of the arm, general coldness and lividity of the surface, pulse almost imperceptible, respiration slow and laboured, countenance haggard and distressed. Previous to my arrival they had administered whiskey in very large quantities, and had immersed the hand and feet in hot mustard and water. Still the pain was excessive, and there was little or no reaction. I continued the whiskey in large doses every fifteen minutes, warmth to the surface, and gave the following preparation, viz.: Bromidi potassii ℥ss; Aquæ ℥viij. Take a tablespoonful every hour until the pain is relieved and sleep ensues.

I remained some hours with my patient, pushing the stimulants and bromide of potassium in fifteen grain doses every hour; in about four hours reaction came on; the surface became warmer; pain diminished, and she slept tranquilly. The arm was still very much swollen. I then left, directing the bromide to be resumed as soon as she woke until the whole was taken. Upon examining the thumb I perceived four distinct impressions of the teeth of the snake in its pulp, which eventually sloughed out, leaving a hole as large as the end of the tip of a cigar down to the bone, a part of which eventually necrosed and came away.

25*th*. Next day, some fourteen hours after the accident, I found that patient had taken all the bromide; was free from pain, and reaction entirely established; arm still much swollen, and she was very weak; had slept some four hours.

26*th*. Still improving; no pain, but much stiffness and tumefaction of the entire right arm up to shoulder; great prostration; pulse slow and weak; some appetite; ordered whiskey occasionally and rest. This case continued slowly improving for a week, when she was enabled to ride on the railroad some fifty miles, and then I lost sight of her; but, with the exception of the sloughing of the tip of the thumb, I heard that she had entirely recovered.

The treatment in this case consisted exclusively of large quantities of whiskey, which has for many years been employed in this section for snake bites, and bromide of potassium in addition. It is true that many cases of rattlesnake bites recover under the exclusive use of alcohol, but many die in spite of its employment, and the pain is always intense for some time. The bromide in this case promptly allayed pain, produced sleep, and seemed to cut short the dangerous symptoms. I am satisfied that she would have died had she been left to the stimulants alone, and that the bromide was the more efficient agent.

CASE II. On the morning of the 30th of September, 1870, I was summoned to visit a stout, athletic negro, æt. 24, on a rice plantation, who had been bitten by a rattlesnake of enormous size on the outside of the left knee, showing the imprint of four teeth; the snake escaped, but was seen by a number of the labourers, who all concurred after a rigid cross-examination in describing it as five inches in diameter, of the size of a man's arm, six feet long, and having fifteen rattles, showing it to be of great size and venom. The patient was carried to the negro quarters and laid on a piazza in the open air, as the thermometer was then 80° Fahr. in the shade.

On my arrival he was crying out with intense pain; limb not much swollen as yet; countenance anxious, haggard, expressive of great anguish; no pulse; hands, feet, arms, and legs icy cold; respiration very slow and laboured, apparently moribund; with much difficulty I forced brandy down his throat at regular and short intervals, and applied hot bricks to his hands, feet, and arms. As soon as he could be persuaded to swallow, gave twenty grains of bromide of potassium every hour, until he had taken 240 grains, or half an ounce in seven hours, along with the whiskey. The pain speedily lessened, reaction soon came on; tranquil sleep ensued, and next day he was much better, though weak, and limb very much swollen and stiff.

His recovery was gradual, and at the end of a week all danger had passed. When I last visited him he was still very weak and his leg stiff; no tumefaction in the joint, but inability to move the knee except very slightly.

The beneficial effects of the bromide were very marked in this case, as manifested in the reaction, sleep, cessation of pain, and general amelioration of the symptoms.

The snake was one of the largest and most deadly species, and I fully believe, that, if treated with alcohol alone, death would have ensued. Although *non unus hirudo facit ver*, still, from the striking result of the bromide in these two cases, I would be inclined to try it conjoined with a free use of alcohol in any other that might fall into my hands; at least until something better is proposed.

ART. VII.—*Description of an Epidemic Malarial Colic which prevailed at Iquitos, Peru, in the Autumn of* 1871. By FRANCIS L. GALT, M.D., of Iquitos, Peru.

THE following notes on an epidemic were taken by the writer, under whose observation the disease occurred, in October, 1871; and, in order that the causal relations may be defined, it will be proper to sketch, even though rapidly, the topography of the locality where it was noticed.

The village of Iquitos is located in the northwestern part of the great Amazon basin, some 2400 miles from the mouth of the Amazon itself, and situated immediately on the north bank of that river. It lies in latitude

3° 44′ 15″ S., and longitude 75° 00′ 30″ W. It is built on a bluff, some forty feet high, overlooking the river, which is here some three-quarters of a mile wide. The soil is sandy alluvium with clay. The land slopes, on the land side, towards a small stream which nearly encircles Iquitos at the distance of some two miles, and which empties into the main river some three or four miles below the town. Some quarter of a mile above the village another small stream debouches into the Amazon. The land about the place is several feet above the highest rise of the river level, though in the rainy season the Indian pathways about the woods are almost impassable, and the small stream first mentioned overflows for some distance from its margins. At some three and a half miles to the southwest is a lagoon which has an outlet into this stream. A very dense growth of forest timber and an almost impenetrable undergrowth pervade this country, even up to the village of Iquitos itself, forming a continuation of the immense low country of the Amazon valley. To the south and east and southwest, this valley extends for hundreds of leagues, being cut through by the large river, and the largest affluent of the Peruvian Amazon, the Ucayali. To the north and west the spurs of the Eastern Cordilleras of the Andes are encountered at varying distances of from 1 to 300 miles. Through this dense mass of vine, parasite, and foliage, the sun rarely ever makes its way, and there is a damp coolness, especially about the parts on the river borders, after the waters have disappeared, which forms a striking and pleasing contrast to the heat of the exposed localities, though the extraordinary amount of atmospheric moisture in this upper part of the Amazon makes a marked degree of difference in the sensible as well as actual grade of temperature as compared with the lower Amazon. The range of the thermometer during the year is, from an average made from 9 A.M. to 9 P.M., 25° Cent.: the mercury never falls below 16° at night, and that figure is only to be noticed during some two or three cool days in July or August, which occur on the whole length of the Amazon River. During the rest of the year the night temperature is never below 20°. The wet bulb shows for the year a difference of some three or four degrees with the dry thermometer, though in October, November, and sometimes August and September there may be 5° difference. The quantity of rainfall, though as yet not accurately measured, probably is not over 100 inches, the greater part of which is noticed from December to April. In the dry season, from May to November, there is the greatest difference of the mid-day and night tepmeratures. During the chilly days in July and August cloth clothing is comfortable, the system so used to a constant high grade of heat being easily impressed by a change of some five or six degrees of temperature, which would not be noticed in our more irregular climate. During the dry season the winds which are the most frequent and the coolest are from S.E., the warmest, and those more noticed in the wet season, being the N.W.

The most notable feature of the climate of the Upper Amazon, on the borders of the main river, is its healthfulness, especially its freedom from the presence of malarial fevers—a circumstance which has been noticed by all travellers on the whole length of the Amazon—the universal experience being that it is only when explorers leave the main river, and proceed up to the rocky country from which the affluents flow, that the malarial intermittent or "Qeuciana," as that complaint is known here, is encountered. The writer, in the months of November and December of 1870 and of January, 1871, was on an exploring trip to the head waters of the Ucayali, a stream some 900 miles long, and the universal testimony was to the healthfulness of this river, which should, probably, more rightly be considered as the continuation of the Amazon, owing to its great size. All those with whom he conversed, some of them intelligent Spanish friars, repeated the statement above made, and it was only on the small streams flowing into the Ucayali and Marañon that fever was to be encountered. In a party of some 100 souls who went as high as the Tambo River, some camping out on shore, and all exposed to night air under varying circumstances of humidity and heat, but a single case occurred, and that was in an Indian who had had the intermittent fever some two years or more before on the Huallaga River, an affluent of the Amazon, which flows through the rocky piedmont district of the Eastern Cordilleras. The traders on the Upper as well as the Lower Amazon all bear a like testimony as regards fevers. And though some cases before the time of the epidemic in Iquitos had come under the notice of the doctors here, they were always, I believe, found to be merely repetitions of attack, originally brought on by great exposure during their trading operations on the small streams leading into the Amazon at one point or another. Dysentery, which in the tropics is so often to be found as a sequel of malarial disorder, is comparatively infrequent, and, as far as I have seen in my short career of eighteen months here, very easy of control; and I am informed to the same effect by the government medical officer. It more frequently is to be found as a result of the vicious and wide-spread habit of dirt-eating among the half-breeds or natives on the rivers, and is then generally fatal, as might be anticipated. Considering the poor alimentation in this remote region, and the extreme carelessness of the natives, the traveller finds much to wonder at in the remarkable healthfulness of the dwellers on the Upper Amazon, the complaints apparently being very much more the result of personal imprudence than arising from any climatal causes of bad health.

The village of Iquitos itself when Hernern came down the river, was an Indian settlement of some 300 souls. The government of Peru, however, in 1864 established workshops here for the repair of steamers, and made it the headquarters of its military authority on the Peruvian Amazon. This, of course, attracted traders here. The workshops were supplied

with English machinists, the Portuguese traders from Brazil drifted towards the place, and it now has a population of some 2000 souls, divided among Indians, Mestigos, Portuguese, white Peruvians, English, and an occasional Yankee; at least four-fifths of the whole being half-breeds of Indian and white of the various nationalities. The houses are nearly all built of cane plastered with mud, and generally whitewashed. For the most part they are thatched roofed, and are strung together, as it were, to avoid expense of additional walls—one story high, having often partitions reaching some four-fifths of the distance from the floor to the roof, leaving a large ventilating space overhead. The streets are wide enough, but without pavements, and are mostly overgrown with grass, and in the rainy season they are filled with water which lies on the ground for weeks together and soon becomes covered with green slime. Into the streets, or the backyards, indifferently, are deposited the filth, human and animal, and a confused smell of nastiness pervades the whole village, the backyards being for the greater part of the year sloppy, often unsunned, and foul with every want of cleanliness, and are generally disgusting in the extreme.

There is no system of drainage adopted, and the water forms standing pools until it has slowy evaporated. In front of some of the tenements of the more sensible of the community, the grass is kept down by frequent cutting, and a patch of cleared dry soil forms a pleasing variety to the overgrown thoroughfares. There has been as yet no well-directed and well-sustained effort to keep the place clean or dry, though it could easily be done with a little system on the part of the proper authorities, and a continuous enjoyment of good health and tropical apathy combined contribute to make every one indifferent to the smells and sights which attract the notice of the most cursory observer.

Towards the close of the wet season, and about the beginning of the same, there is a somewhat more notable increase in the sickness. About June, July, or August, catarrhs are more common, and especially attract the notice of the public. During the last year at that time an epidemic of influenza extensively prevailed in Iquitos, attended with severe symptoms, though no fatal case occurred. About October dysentery is more to be noticed, and fevers among those who have been previously exposed elsewhere to attacks, but there is no severity in the symptoms, and, in fact, taking into consideration the want of public hygiene, it would be difficult to find anywhere in warm countries a place more free from severe forms of disease than is Iquitos. It is not improbable that in the course of time—a long way off, however—as the forests become cleared off and agriculture turns up the soil of this Amazon valley, there may be a large development of malarial disorders. As yet the heavy rains and floods, which act as natural scavengers for the decaying animal and vegetable filth, are a good police for the Amazon basin, and the unsunned forest keeps

the whole surface of the earth below a temperature necessary for fermentation of decaying material.

The general condition of good health in Iquitos, however, as well as above and below the river from here at some of the Indian settlements, was during the past year notably interrupted. As before stated, epidemic catarrh was prevalent here, and at other places near here, about July. After that disappeared—it lasted about six weeks—there was nothing unusual in the sanitary condition until about the 10th of October; the previous month or two, as said before, being unusual for dryness, high heats, and great differences in the day and night temperatures. The symptoms noticed in the first cases repeated themselves so exactly in some twenty or thirty others as to have justified the opinion of there being some epidemic cause of disease in the population of Iquitos, and those attacked were of the various nationalities to be found here, and the cases were all those of adult age, with probably two exceptions. The cases all occurred within some four or five weeks, with one or two exceptions, which appeared in December—long after the force of the epidemic had expended itself, and it originated apparently some 200 miles down the river, and recovered here under treatment. In three cases the characteristic symptoms supervened in the course of other diseases. One of the cases came here sick from the Huallaga River, some 200 miles above Iquitos, and was, with one exception, the most violent noticed, the active stages lasting longer than in any other patient, and the recovery more *rapid*.

In Grisolle's Pathology, under the head of "Colique Vegetale," the reader will find the symptoms of the disease very applicable to those noticed in the epidemic here, and will also see the synonyms of the complaint stated, such as "Colique de Poiton," &c. He will also, in the most recent edition, find that the author changes his opinion expressed in a former, and gives as a cause for these symptoms the presence of lead in the system of the person attacked. He will also there see that a writer, M. Fonnsagrives, thinks that under the influence of miasms analogous to or identical with the "miasmes palustres," the same state of things might be engendered. The probability of the truth of this suggestion is almost made a certainty in the epidemic which I shall now detail as briefly as possible.

There were in about half of the cases certain prodromata, though not always well marked, and which would never have attracted the patient's attention by themselves. Such were an indifference about eating, a vague sense of lassitude, inclination to sleep, disposition to sluggishness of the bowels, and irregular and rapidly passing chilliness of the surface, occurring and recurring very irregularly in the twenty-four hours. This state of things would be noticed some four or five days before the patient sought medical advice. The first well-marked symptom for which relief was asked was pain in the abdomen, located by the patient at the epigastrium, or more

frequently about the umbilicus, shooting throughout all parts of the abdomen, and oftener from before backwards to the spine, coming on without reference to meals, and in a few hours amounting in all cases to great torture, causing frightful contortions of the body, ceasing for a moment or perhaps ten minutes, and then suddenly recurring. The agony in most of the cases was pitiable, the patient begging sometimes that something might be given to put him to sleep forever, and the successive attacks produced a most disagreeable feeling of dread at the anticipated return. The pain was not affected by pressure, never made worse, though now and then it afforded slight temporary relief. It was noticed in some of the cases that in these violent paroxysms of pain the sudden entrance of the medical attendant, or diverting the attention of the patient, would have the transitory effect of quieting him.

Nausea and persistent vomiting accompanied, sometimes preceded, the pain, and prevented often the administration of medicines for hours. The matters thrown up when the stomach was not distended by previous meals consisted of bilious, watery ejections, and in two cases the colour of the fluids were of a very pale *India ink* colour. Relief to the pain from vomiting was very fleeting. With this there was not the slightest febrile action of the pulse; the only frequency it showed was the result of the violent contortions of body during the pains or the vomiting. When the patient was quiet, the pulse was perfectly natural in every respect. In some five cases there were exceptions to this in an acceleration towards evening, and it was also noticed after the pain had lasted some two or three days that towards evening there was an increase in the violence of this symptom, the patient during the early morning being always better. On the second or third day of the disease there began to be noticed a yellowness of the conjunctiva; in two cases this was one of the premonitory signs, and this yellowness continued in an increased degree during the remainder of the attack, and some days after convalescence. In some of the cases the whole body had that peculiar bronzed and splotched appearance so marked in bad cases of yellow fever.

There was a singular freedom from headache during the whole of the disease, the mental faculties never at any time being in the least affected. Close questioning of the patients revealed that they felt at times, irregularly, a chilliness passing rapidly about the person, not having any relation to particular times of the twenty-four hours, except in three cases when this sensation was noticed towards evening more than at any other time.

It may be stated here that about one-fourth of the cases were pure-blooded Indians from whom it was difficult very often to get any account of sensations of any sort, the peculiar apathy of the race making them very poor subjects at a cross-examination.

In about one-fourth of the cases the patients had, at previous times,

been subject to chills and fevers contracted on their various canoe trips on the tributaries of the Upper Amazon; in one case, that of a Londoner, he had some years before taken quinia, apparently for some intermittent febrile condition.

In all cases the urine was high coloured, without albumen and clear—and this remained among the last symptoms of the disease.

The constipation was present in all cases, and extremely obstinate, seeming to defy all attempts at resolution by means of purgatives, whether given by enema or by the mouth. Attempt at purging was one of the modes attempted to relieve the disease at first, until the complaint seemed to be better understood, and it was wonderful sometimes to see what little effect violent drastics had. In fact, this constipation passed off with the disease independently of purgatives, the patient being relieved of it without any remedies applied for that particular object. The only good derived from enemata was the passing off large quantities of disengaged gas, which afforded temporary relief, flatulence being one of the most prominent and troublesome sequences of the costiveness and apparently paralyzed condition of the alimentary canal. Eructations were very annoying and often most offensive. The usual remedies for this symptom did not seem to be productive of any good effect.

The tongue was in all cases coated, generally yellowish-white—the colour of the tongue itself being a pale pink in most of the cases.

Prostration of the system was early induced by the constant pain and vomiting, and sometimes reached a serious point of debility; even after the active character of the complaint had disappeared, there was a long and tedious convalescence, somewhat owing, 'tis true, to climatal lassitude, and also to the very poor alimentation of this remote wilderness, which made it very difficult to get the proper diet for invalids. There was a notable complaint on the part of the patients of a "sinking sensation," referred to the epigastrium, accompanied often with a disagreeable abdominal palpitation, which troubled the patient for some days after convalescence had begun.

In about one-half of the cases, there was slight perspiration—in two it was profuse, and seemed to come on during the night.

In one of the cases, the most protracted of all, the patient had diarrhœa about a week before the beginning of the epidemic disorder. The diarrhœa was suddenly checked, and the pains began the following day.

There were no fatal cases, though several of them excited grave apprehensions, from the prostrating effects of pain and debility.

In commencing the treatment of these cases, the most obvious symptoms which called for relief were the excruciating pains and the vomiting, the character of which so resembled the ordinary colicky pains that anodynes and revulsives naturally recommended themselves. The medical attendant found himself, however, startled by the want of relief to both by the reme-

dies used; there was very great toleration to the use of opium, and unusual quantities of its various forms seemed almost useless in giving the desired relief; for a moment or half an hour the pains would be allayed, and the patient, refreshed possibly by a little sleep, would cry out suddenly with acute repeated paroxyms of the agony, which would pass off partially in half an hour, leaving him further prostrated by this and the repeated vomiting combined. It sometimes happened that the stomach became quiet in the course of some twelve hours, though the pains continued. The greatest relief experienced was by the use of small blisters to the epigastrium, which were sometimes sprinkled with morphia, though really the blister alone frequently did as much good as when followed by the opiate. It is not improbable that in some cases the flatulence was increased by the use of large amounts of narcotics. Carminatives, given to relieve the flatulence, and antacids seemed to be of little use, and, in fact, no medication applied with a view merely to the most conspicuous symptoms seemed to reach the bottom of the matter. The slightest thing taken to relieve the thirst, which was, by the by, a very marked symptom, increased the feeling of distension about the stomach and bowels, and there was a disgust for food of every sort.

The above is a sketch of the general plan of treatment used in the first three or four cases, which progressed poorly enough, the pain, anxiety, constipation, and flatulence continuing, causing sleepless days and nights to the patients and their friends.

It then was judged that, since there was a tendency to increase of severity of symptoms towards night, that now and then, about the after-part of the day, a vague chilliness was noticed, and taking the topographical position of the place into consideration, the administration of quinia should be experimentally tried, conjoined with calomel at first. The effect of this treatment made itself manifest surely and with reasonable rapidity. Valerian or belladonna was conjoined to quiet nervous excitability, the quinia being given to the extent of fifteen to twenty grains in the course of twelve hours. The use of the antiperiodic continued for one day made a notable impression on the attacks of pain, and refreshing sleep succeeded to restless nights. It was found, also, that costiveness, which had resisted "heroic" purging, gave way of itself as the bowels recovered from their paralyzed condition, and a copious purgation was frequently the signal for beginning convalescence. The quinia was continued for several days after the pain had subsided, in gradually diminishing doses, and the anodynes discontinued. The quinia was given in the form of pill generally, the stomach seeming to retain it more easily than when used in liquid form. After the first good effect of the quinia treatment, none other was used during the epidemic by either of the physicians by whom the cases were treated, and the very happy solution of the symptoms under its use was as satisfactory as could have been desired. Nothing particularly was done to relieve the

yellowness of the conjunctiva which sometimes continued in a greater or less degree after the patients had returned to their usual labours. Alkaline remedies were sometimes used with the antiperiodic towards the close of the treatment, when there was any unusual acid eructation. In estimating the nervous excitability which was noticed among the cases as they were seen here, account must be taken of the unusual exaltation of nervous sensibility in all complaints of these tropical countries, and which often is one of the most troublesome attendants on symptoms of diseased action.

Since the disappearance in an epidemic form of this complaint, which may be characterized as a malarial colic, two cases have occurred in December, at different times; and another case, about the last of the year, came here sick, from an Indian village some two hundred miles down the river, with exactly the same symptoms, and was cured under the same treatment.

The commanding officer at the Peruvian frontier, below the village above referred to, had lost two Indians during November, and his description of the cases led me to suppose that there were others of the same type. Not being supplied with either medicines or a physician, the treatment was merely that by domestic remedies.

I am informed here by the government physician, an intelligent Ecuadorian, that violent forms of colic are very common here, especially among the Indian population, but that he had not before seen any case which was not relieved permanently in the course of twenty hours by the use of anodynes and antispasmodics. These colics can be traced to great imprudence on the part of the ignorant, who eat enormous quantities of plantains or half-ripe fruits, and expose themselves to the action of rains, or bathe imprudently. It may be mentioned that in the cases occurring of the epidemic, some of the patients in vomiting threw up worms. The presence of these parasites in the alimentary canal may be considered here as one of the most universal facts of the condition of the people, even among the grown population, and among the children it is sometimes astonishing what numbers of these animals will pass off by the use of proper medicines. Among the adults they rarely seem to trouble the person; but among the young they cause convulsions and other of the ordinary derangements of health. The cause of the presence of these creatures may be found in the poor diet and carelessness of the population. In some cases of colics with icteroid features, I have been led to imagine that possibly these animals might have made their way into the ducts leading into the duodenum from the liver. Unfortunately, the very superstitious nature of the natives and half-breeds of this region has prevented the medical men of this place as yet to make post-mortem examinations, which has rendered the diagnosis and treatment of the disease somewhat unsatisfactory; and the authorities, very naturally, are unwilling to excite the fears or distrust of the natives, who are largely employed as labourers,

as a panic might deprive the village of the presence of the only labouring class, who would run off to their forests on very slight suspicions of evil.

From what I have been able to learn on the subject, there had been, during the latter part of the last year (1871), a greater prevalence of disease of a malarial character than has been heretofore known on the Upper Amazon, though the accounts given have been of such a vague and inaccurate nature, that it is difficult to arrive at any conclusion as regards the type of disease said to have existed. In only one instance was anything definite learned. At an Indian village, some two hundred miles above here, immediately on the river, a severe form of bilious fever seems to have prevailed about July last, which, from the panic it produced, caused the desertion of the place. It had a population of some 250 Indians, with one white Peruvian as governor. His wife, also a white Peruvian, came to Iquitos for treatment, herself being then ill with a severe attack of the disorder of some three weeks' standing. From her I learned, that, when the disease first appeared there, the natives, supposing it to be ordinary intermittent fever, with which they were more or less familiar, applied their native herbs in common use, and, finding them useless, and that several died after some eight or ten days' sickness, were seized with a panic, and deserted the place completely, leaving the sick to their fate. In the case of the lady herself, the symptoms seemed to be, as far as I could learn, those of a bilious remittent fever, in which there was conjoined a dysenteric complication. When she came here she was excessively prostrated, having febrile accessions towards evening, and on several parts of the body there were troublesome boils which had been reappearing and healing for some two weeks. I was told that in some of the cases there was cough with bloody sputa, and a delirium which early supervened, and which led me to think of typhoid pneumonia. The lady regained her health under the long use of quinia and ferruginous tonics, though convalescence was tedious, and for a long time there was a great sense of prostration about the stomach, and a feeling of burning, with want of appetite.

This epidemic on the Maranon (or Peruvian Amazon) seems, so far as I can learn, to be altogether exceptional, and the exemption from malarial fevers on the borders of the "King of the Rivers" is truly remarkable, though one will have to take into account, in judging of this matter, the hitherto remoteness or sparsely settled nature of the country, the unreliability of the information received, arising from the ignorance of those living along the borders of the streams, or the mistakes which unprofessional travellers might make on this subject. Still, with all this reserve, the margin of the upper Amazon presents itself in most favourable contrast with other tropical river bottoms, and those lying adjacent to such warm latitudes.

It is hardly necessary to say that Grisolle's opinion about the origin of the series of symptoms such as were observed in the epidemic in Iquitos

could have no possible relation with the presence of lead in any form whatever, the treatment itself also affording sufficient additional proof, probably, if any were wanting. I was, as the result of my experience, led to believe the epidemic one of a malarial disorder, which seated itself possibly in the solar plexus, and essentially of a neuralgic character arising from the "miasmes palustres." Why the malarial emanations should show themselves in such a "questionable shape" I do not pretend to say.

IQUITOS, Peru, Jan. 16, 1872.

ART. VIII.—*Remarks on the Use of a Rectilinear Écraseur, in the Removal of Hemorrhoids, the Cervix Uteri, Penis, Tongue, Nævi, Folds of the Vagina, &c.* By J. C. NOTT, M.D., of New York. (With a wood-cut.)

AT the request of Messrs. Tiemann & Co., my instrument-makers, I, some months ago, published a brief sketch of this instrument in the *New York Medical Record*, and now at the suggestion of Prof. J. T. Gilmore, who sends me his notes of some interesting cases operated on by himself, I am induced to place it somewhat more prominently before the profession, under the conviction that the instrument is one of practical utility and susceptible of extensive application.

The following note, dated Mobile, 17th February, 1872, from Professor Gilmore, one of the best surgeons I know at the South, will speak for itself:—

"J. C. NOTT, M.D., *Dear Doctor:* I send you a report of several cases operated on with your improved *écraseur*. I have used it in all the cases of hemorrhoids I have been called on to treat, that required operative interference. I have the records of four cases, but as they possess no special interest in addition to what you have already published, I omit any mention of them. The notes of the cases inclosed were carefully kept and the reports drawn up by Dr. Rhett Goode. I have some half a dozen times removed the penis with Chassaignac's *écraseur*, and have seen it followed by troublesome hemorrhage. The two cases operated upon with your instrument *did not bleed a drop*.

"For the removal of the *cervix uteri*, in the case in which I used it, it could not be excelled. I, however, operated on another, one of cauliflower excrescence, on Friday last, in which it was not large enough. The clamp being too short, it would not embrace the whole cervix, which was enlarged; and, the woman being obese and the uterus high up, I found it impossible to apply it. I therefore used Chassaignac's instrument.

"In sound tissues, I think your instrument a reliable safeguard against hemorrhage. In all cases in which I have employed it, *the tissues were simply crushed*, leaving them for division by knife or scissors, or the application of ligature.

"Were I in your place I would publish the instrument in the *American Journal of Medical Sciences*."

The *écraseur linière* of Chassaignac, long and favourably known to the profession, though extremely valuable in appropriate cases, is not free from objections. 1st. It is occasionally followed by serious hemorrhage, particularly in internal piles, which is embarrassing and dangerous. 2d. From the dragging action of its chain or wire, it is apt to draw in more tissue than is desirable. For this reason it has several times, in the hands of experienced operators, while removing the cervix uteri, made openings into the peritoneal cavity or bladder. And in operating on hemorrhoids, by drawing in too much tissue, it has been followed by stricture of rectum. 3d. It is very tedious in its operation, particularly where several hemorrhoids are to be operated on in succession. 4th. Several sizes and shapes of this instrument are required for different cases, all of which may be easily managed by one *rectilinear* instrument. 5th. Chassaignac's instrument is only applicable to *circular* masses of tissue, whereas my instrument will grasp and remove, with the precision of a pair of scissors, any strip of tissue large or small, which is a great advantage, particularly in the operations of elytrorrhaphy and hemorrhoids.

These objections led me, two years ago, to the invention of my instrument, and the experience of other surgeons, as well as my own, justify me in saying that they are to a great extent, if not wholly, overcome by the *rectilinear écraseur*. It is applicable, I believe, to all cases where the *écraseur* of Chassaignac can be used, except in intra-uterine polypi, and the neck of the uterus in very deep or narrow vaginæ. In such cases there is not room for manipulating.

A glance at the accompanying cut will show that this *écraseur* has two *parallel* blades, coming together in a right line, like a clamp. The blades are about

three inches long, but may be made of any desired length. One has a fenestra running its whole length; the other presents a thin blade, about a line in thickness, with small saw-like teeth on its edge, and it is so constructed as to pass into and fill up pretty closely the fenestra when the clamp is closed.

The part to be operated upon is seized with a volsella, or forceps, and put gently on the stretch; the clamp is then applied at the point indicated; it is next screwed down firmly, as far as the screw will press the blades. A part of the tissues are driven by the saw blade deep into the fenestra, and the shoulders or ledges, projecting from the sides of this blade, compress and crush the tissues for an additional line on each side—thus guarding more effectually against the danger of bleeding.

This instrument does not, like that of Chassaignac, *sever* the tissues, but, after crushing them down to a bloodless, flattened, translucent, insensible pulp, in the track of the clamp, leaves the part to be treated in one of two ways, according to circumstances. 1. All the strangulated mass projecting above the clamp may be shaved off closely; or, 2d. The clamp may be removed after compression, a ligature be tightly tied in the sulcus formed by it, and the part projecting beyond the ligature then cut away.

The tissues are so compressed that a very small, deadened pedicle is left for ligature. The pedicle, for example, of a large internal hemorrhoid, when tightly tied, will not be larger than a goose quill. In operating with the *écraseur* of Chassaignac, the part is entirely cut through, consequently leaving a very thin margin of crushed tissue to protect against bleeding, while the rectilinear *écraseur* leaves a much broader mass of mangled tissue to rely upon.

There is a practical point in connection with the use of the ligature, in conjunction with my *écraseur*, which must be decided to a great extent by the judgment of the operator. In general terms I may say that in tissues with a good deal of resistance under this instrument (such as the cervix uteri, penis, folds of vagina, &c.) no ligature is required after it, to prevent bleeding. On the contrary, the opposing surfaces are so closely approximated, and so entangled or dovetailed together, that they *do not separate when the clamp is removed, or require retentive stitches.*

The case, however, is different, I think, with regard to *internal* piles. These are composed of peculiarly soft, vascular, hemorrhagic tissues, which pour out blood on the slightest provocation. In operating, therefore, upon internal piles, I first apply the clamp firmly for a minute or two—then remove it—apply a ligature in its track, and snip off the projecting mass. The sensibility of the nerves is so deadened by the crushing, that a ligature gives little or no pain, and makes surety doubly sure without being liable to the slightest objection. If my instrument were kept applied as long as that of Chassaignac's usually is, it would afford at least quite as much security as the latter, but it is unwise to trust either in this position.

In the removal of *external* piles, on the contrary, those lying upon and without the sphincter, I crush, and snip off unhesitatingly, without ligature.

My experience, as yet, is very limited in the application of the *rectilinear écraseur* to removal of portions of the vagina in operations for procidentia, cystocele, rectocele, &c., but I am greatly mistaken if this will not prove to be its more important use. The operation of elytrorrhaphy is a difficult one, and none but specialists can do it with encouraging results, by the delicate clipping and stitching processes of Sims and Emmet. The operation must be simplified, to place it within the reach of the profession generally. In two cases, in which I have removed portions of the vagina with my *écraseur*, I was quite unprepared for the results which followed. One of these cases was operated on in St. Mary's Hospital, in Brooklyn, through the kindness of Dr. John Byrne, chief surgeon of the hospital, in his presence, as well as that of Dr. Sims and other medical gentlemen.

The folds of the vagina in each of these cases were seized by the *écraseur*, which was then firmly screwed down; the projecting parts of the folds were cut away with scissors, and the clamp removed to make way for interrupted silver stitches; to the surprise of us all, *the tissues did not separate when the clamp was taken off*, but, on the contrary, were so firmly glued together that no gaping occurred; no stitches were applied, and immediate union followed without suppuration. It will be seen below that the same result followed in the case of amputation of the cervix uteri by Dr. Gilmore with my *écraseur*. In none of these cases was there the slightest hemorrhage, and such is the usual effect of this instrument. Now if these facts are confirmed by further experience, it is clear that this instrument enables us to greatly simplify many vaginal operations.

The only operations for hemorrhoids now much in vogue are: 1st. The ligature with or without division of the lower tegumentary margin of the tumour. 2d. The clamp with actual cautery. 3d. Galvano-cautery.

The two latter are tedious, require troublesome preparation, and are more or less expensive, as well as alarming to the patient. The ordinary operation by ligature, as recommended in our books, requires about the same time as the rectilinear *écraseur*, is more painful, and the cure is more tedious. Where the pile is first crushed, the sensibility is much less, and the ligature cuts through in shorter time.

I will now give a few typical cases to illustrate more fully what I have said. After having operated on a good many cases previously, about a year ago, I thought the following one would put my instrument fairly to the test. Having decided to operate, I invited to be present Drs. Ed. L. Keyes, Fred. A. Castle, Leroy M. Yale, and B. W. Dudley.

CASE I.—Mr. S., aged 40, had suffered greatly for many years with hemorrhoids, which always protruded largely at stool, and often bled copiously. He was feeble, emaciated, anæmic, dyspeptic, no appetite, nervous system greatly deranged, and seemed the mere wreck of a once good con-

stitution. An enema was given preparatory to the operation, which brought out a mass almost as large as the fist, on which were counted five tumours, entirely within the sphincter, varying in size from a nutmeg to a large walnut, and completely encircling the gut. The patient was then put under the influence of ether; but I may remark in passing, that I have often operated on *internal* piles by the *écraseur* with very little pain, for if screwed down quickly, the nerves are crushed and deadened.

Everything being in readiness, one of the piles was seized with a volsella and drawn gently out; the clamp was then quickly applied to its base, and screwed down tightly; after a minute or two the clamp was removed, while the pile was still held by the hooks, and a ligature applied tightly; all the pile beyond the ligature was then snipped off, except just enough to retain the ligature. The remaining four tumours were successively treated in the same-way, and the whole operation did not consume fifteen minutes. The protruding part was returned into the gut. After the effect of the ether passed off, the patient complained of no pain—merely a sensation of soreness; but, in order to insure rest, I gave morphia hypodermically. I had never seen so little suffering from an operation of this kind, nor so rapid and satisfactory a recovery as followed.

The piles in this case were so numerous, large, and vascular, and the patient had been so drained of blood by repeated hemorrhages, that it would have been folly to trust the *écraseur*, without ligature, when so far from being objectionable, it could only hasten the cure, by insuring against hemorrhage, and by causing the pedicle to drop off sooner. If the part be well crushed by the *écraseur*, the ligature gives little pain, and reduces the pedicle to its minimum.

CASE II.—Between two and three months ago, a case even worse than the above came under my charge. The subject was a lady 60 years old, who had been annoyed by hemorrhoids for thirty years. She was the wife of a prominent homœopathic physician, who did not believe in surgical interference in such cases. She was naturally of extremely nervous temperament, and her malady had worn upon her until she was a pitiable wreck. For many years she had not had a stool without protrusion of the piles, accompanied by more or less pain and difficulty in returning the mass; she had exhausting hemorrhages, was pallid, excessively nervous and hysterical, dyspeptic, and without appetite. She had been always able to push up the protruded mass after stool (though often with much difficulty), until five days before my first visit.

On examination, I found an apparently desperate condition, both local and constitutional. She was so nervous and so sensitive to the touch, that she shrunk from every attempt at local treatment, and could not take opiates. The protruding mass was about the size of a small orange, and almost black from strangulation. After temporizing two days with puncturing, emollient, anodyne, and astringent applications, it was evident that the whole mass would slough, if allowed to remain *in situ*. I accordingly put her under ether, and with much difficulty succeeded in pushing it up.

For a month, her suffering was intense, from the inflamed and irritable condition of the entire circumference of the rectum. She was obliged to have a stool once in three or four days, which had to be obtained with great difficulty by enemata, her stomach not retaining laxatives of any

kind. Whenever the stool took place, the hemorrhoidal mass was forced out, and ether had again to be given for its reduction.

Matters went on in this way for the month, and I saw she must die from the wear and tear, if allowed to continue. Her stomach would not retain food, and she was becoming more feeble every day.

Having made up my mind to the necessity of a surgical operation, in spite of the unpromising symptoms, I requested Dr. Wm. H. Van Buren to see her in consultation. He at once agreed with me as to the propriety of operating, and the operation was accordingly done, with the assistance of Dr. Van Buren, and that of Dr. J. Marion Sims, and Dr. Geo. B. Fowler.

An enema was given, which brought a mass down about the size of a large lemon; there were three internal tumours, the largest of which was fully two and a half inches long, nodulated, hard as a uterine fibroid, and covered with superficial ulcers. She was then fully anæsthetized. Dr. Van Buren seized the largest tumour with a volsella, and held it while I adjusted my *écraseur* on its base, and screwed it quickly and firmly down. After two or three minutes, the clamp was removed, a ligature tied firmly around the base in the sulcus formed by the instrument, and the whole mass cut away with scissors, leaving only enough to prevent the ligature from slipping. The other two tumours were removed in succession in the same way; the protruding gut then returned within the anus, and morphia given hypodermically, though she does not bear opium well.

This case was the most aggravated one I think it has ever been my lot to treat, and from the condition of the rectum and nervous system, it is not a matter of wonder that a good deal of suffering should continue for some days. I saw no evidence that the crushing and tying the piles added anything to the suffering, which was rather less than it had been previously. On the second day she was evidently less uncomfortable, began to take nourishment, and got more sleep. On the fifth day she had a copious moulded evacuation without any protrusion of the bowel, and with less pain than she had had for many weeks. The progress of the case since has been steady and satisfactory. She now, at the end of six weeks, is fairly convalescent, sleeps well, eats well, and has evacuations without trouble. The only drawback is from two small external piles, which ulcerated, and which it was thought most prudent to leave untouched at the time of the operation, as we did not wish to risk any additional shock to the constitution in her nervous, prostrated condition. She is quite delighted with her present condition and prospect of future comfort.

I could add other cases from my own practice and that of others if there was any object in doing so. Dr. T. Gaillard Thomas informed me that he had used the *rectilinear écraseur* with much satisfaction; and it has been tested by Dr. J. Marion Sims, Dr. B. W. Dudley, and others.

Professor J. T. Gilmore, of Mobile, has kindly sent me his notes of the following cases, operated on by himself; but, having already consumed more space than I anticipated, I must be content with abstracts.

CASE III.—D., 24 years old, patient in the City Hospital—abscess of testicle which completely destroyed the glandular structure, was opened—threw out a fungous growth and became completely everted. There was no remedy left but extirpation of the organ. After separating the gland from the surrounding structures and isolating the cord, the latter was

crushed with Dr. Nott's *écraseur*. As a means of precaution a ligature was applied in the track of the instrument after its removal. The cord was then divided with a scalpel near the ligature. There was no hemorrhage, and the patient made a speedy recovery. The ligature came away on the fifth day.

CASE IV. *Amputation of Cervix Uteri.*—In the last days of November I was invited by Dr. Sherrard to operate on a patient of his, M. N. (col.), aged 29 years. She had never borne any children, although she had been married for ten years and had menstruated regularly. On examination, that peculiar condition of the uterus described by Huguier as hypertrophic elongation of the cervix, to which he exclusively ascribes complete prolapse, was found. In the standing posture the cervix hung out of the vulva more than an inch. The patient, under chloroform, was placed on her left side, Sim's favourite position for uterine operations. Dr. Nott's rectilinear *écraseur* was applied, grasping the cervix evenly close to the vaginal insertion. The clamp was rapidly screwed down and allowed to remain for some five minutes, and, before removing the clamp, with a pair of scissors I removed the strangulated cervix as close to the clamp as I could. *On removing the instrument there was not a drop of blood*, and the crushed tissues were not cut at any point. The woman remained in her bed for five days, and then in her room for ten days longer. Dr. Sherrard informed me that *the approximated mucous membrane covering the stump did not subsequently separate, but healed by immediate union. At every part the covering was more uniformly and evenly applied than it was possible to have done it by sutures.*

P. S. I have just examined the cervix, *and find the os well preserved* and its vaginal surface free from cicatricial formation. The vaginal mucous membrane has accurately covered it.

CASE V. *Amputation of the Penis with Dr. Nott's Rectilinear Écraseur.*—D., City Hospital, age 103 years. Epithelial cancer of glans penis. The patient having been put slightly under the influence of sulphuric ether, the penis was removed within an inch of its attachment to the pubes. The diseased mass and that part of the penis external to the instrument was removed with a knife. The *écraseur* was allowed to remain for a short while, so as to thoroughly crush all of the vessels. Upon its removal there was not a particle of hemorrhage. *No ligature was applied.*

CASE VI. *Epithelial Cancer of the Penis; Amputated with Dr. Nott's Rectilinear Écraseur.*—M. D. (col.), age 50 years, from Mississippi. No hemorrhage. The patient returned to his home one week after the operation.

This instrument has been applied satisfactorily to the severance of the adhesive bands in the operation of ovariotomy, and, although it has never been tested on the pedicle of an ovarian tumour, I cannot doubt the safety of the principle in such cases. In large pedicles it would require a larger and stronger instrument, but one could be made, I have no doubt, that would divide all the soft parts (in an amputation) below the knee, without the loss of an ounce of blood. I amputated a finger with the one I use, through bone and all, leaving the flaps firmly glued together and requiring

no ligature or stitch. There was not a drop of blood. The stump was round and smooth, looking like a cut stump in the last stage of healing.

After I had conceived the idea of a *rectilinear écraseur*, and had one manufactured by Messrs. Tiemann & Co., I exhibited it to my friend, Prof. Isaac E. Taylor, under the conviction that the idea was peculiar to myself. To my surprise, he brought forward one he had invented twelve years previously, identical in principle, though differing in shape and some minutiæ of construction. His was *curvi*-linear, mine *recti*-linear, &c. His instrument, though invented twelve years previously, was never published that I am aware of, is not exhibited for sale by the instrument-makers of New York, and having been a resident of this city but four years, it is by no means strange that I never heard of Dr. Taylor's instrument until it was taken from a private drawer in his office and shown to me. His instrument, I believe, was particularly designed with a view to removal of uterine growths. I think, though perfect in principle, it is of inconvenient shape, and applicable in few cases in which an *écraseur* can be used. It is to be regretted that the doctor did not push his idea through to its legitimate results.

ART. IX.—*Two Cases of Rare Disease of the Tongue.*

By J. H. POOLEY, M.D., of Yonkers, N. Y.

CASE I. *Abscess of the Tongue.*—February 24, 1869, I was called to see a little girl seven years old. Two days before my visit the child first complained of pain in eating and difficulty of swallowing, and the mother noticed a swelling of her tongue. The patient appeared to be pretty well, there was no fever, no excitement of pulse, she looked a little paler than usual, and seemed rather weak. Upon examining the tongue it was found slightly coated in the middle, and red along the edges; about the middle of the left lateral lobe of the organ there was a smooth ovoid swelling, about the size of a pigeon's egg, shining, as if denuded of epithelium, intensely red, with a few little yellowish-white dots here and there upon its surface; it was but slightly tender on pressure, and gave an obscure feeling of fluctuation. I proposed to puncture it, but the child objected very strongly, and as the case was not urgent I did not insist. The next day it was about the same, it gave the child very little inconvenience except when she attempted to eat. Her diet was confined to liquids and slops. The following day the mother informed me that the swelling had burst early in the morning, and had discharged a tablespoonful of matter, it was very much reduced in size, and a little pus was seen oozing from a point on its surface, in a day or two more it entirely disappeared, and the child was quite well.

Erichsen says: "Abscess of the tongue, though rare, occasionally occurs. A boy was brought to me sometime ago with an elastic fluctuating tumour of slow growth and about the size of a small plum, situated deeply in the

centre of the tongue; on puncturing it, about half an ounce of healthy pus was let out, after which the cyst speedily closed." Other similar cases may be found scattered through medical literature; several are referred to in *Holmes's Surgery*.

CASE II. *Fibrous Tumour of the Tongue.*—August 8, 1870, I was asked by her family physician to see Miss C., æt. 23, on account of a tumour, as he stated, of the larynx, which was interfering with respiration and deglutition, and giving rise from time to time to serious hemorrhages. I found her quite pale and anæmic, but otherwise, with the exception of her local trouble, in good health. She stated that about a year before she had began to notice some obstruction in her throat, to which at first she paid but little attention, as she regarded it as the result of an ordinary sore throat, but it steadily grew worse, until she was compelled to call in a homœopathic practitioner, under whose treatment she had been for some months. A few days before my being asked to see her he had discovered for the first time the existence of a tumour in her throat.

Upon depressing the tongue in front of a good light, I could plainly see projecting above the base of the tongue the top of a round, smooth tumour apparently about the size of a pigeon's egg. Upon passing my finger into the mouth the tumour felt very firm, hard, and incompressible, and was plainly made out to be situated at the base of the tongue, in part at least, for it was situated so far back that, though I introduced two fingers, I could not fully circumscribe its base or peduncle, which seemed to be quite large, and I feared might in part involve the edge of the larynx. The tumour was not sessile, but had a distinct though broad pedicle. It interfered seriously with deglutition, so that for some time the patient had not been able to swallow solid food, but it was not painful and did not impede respiration to any important extent, or interfere with speech; there had been three somewhat severe bleedings from it. My protracted and somewhat rough examination did not cause any hemorrhage from it. I prescribed iron, and a gargle of bromide of potassium, from its reputed anæsthetic effect upon the fauces, and advised her to go to New York to have a laryngoscopic examination made of the growth. She promised to do so as soon as she should have somewhat recovered her strength; but while waiting for this she was attacked with a very severe hemorrhage from the tumour, to the extent of producing syncope. No accurate idea could be found of the amount of blood lost, as a good deal of it was received on towels and cloths, though the friends said that for several minutes it poured in a stream from her mouth. The bleeding was stopped by the local application of liq. ferri persulph. of full strength, and she was advised to have the tumour removed at once, but decided to put it off, as she thought she was too weak to bear it. Hemorrhages recurred from time to time at intervals of a few days, or a week, which were arrested with the persulphate, and the advice to have the tumour removed repeated, but still she kept putting it off. On one occasion I went with my instruments, prepared to operate, but again she declined, saying she was too weak.

At last, the intervals between the attacks of bleeding becoming shorter and shorter, and a fatal termination being threatened, she consented, and on Sept. 4th, ably assisted by Dr. J. Foster Jenkins, I proceeded to operate. Ligature was hardly to be thought of, knife or scissors I was afraid to use, even if I could have reached the base of the tumour with them, which, however, was impossible.

I had, therefore, provided myself with a wire *écraseur*, with a suitable curve at its extremity, and after several trials I succeeded in snaring the tumour in the wire loop, and pushing it down as far as accessible with my finger, for most of it was out of sight and reach, I began slowly to tighten; but the base or pedicle was so unexpectedly broad and hard that the wire broke, and as it broke near the loop, the instrument had to be removed and a new wire adjusted. Upon tightening again, after a few turns this second wire broke, fortunately near the shank, it was spliced, and we proceeded; it broke again; the instrument was now removed again and a thicker wire rope, double the thickness of the other two fitted to it. This bore the strain very well for awhile, but at last it broke also, and had to be spliced. This happened several times, till at last just as we were at our wits' end, the tumour was cut through. This operation was performed without anæsthesia, with the patient sitting in a low chair. She nearly fainted several times, was constantly interrupting progress by threatening suffocation, and the friends, discouraged by our frequent failures and her alarming appearance, could hardly be persuaded to persevere to the end. The operation lasted over an hour, and was altogether the most difficult and perplexing that ever fell to my lot. Never did I experience a greater feeling of relief than when the tumour was at last detached and hooked out of the patient's mouth. One source of failure was in the faulty construction of my instrument, the edges of the slit through which the wire loop emerged not being sufficiently smooth and rounded, but when the wire was tightened against them wearing them through, and thus causing at least two of the breakages. This is a point I should carefully look to in a future case of a similar kind, and I think it would be well, too, to be provided with a second instrument ready armed, which would save time in case of breaking the wire and being obliged to apply another one.

The operation was unaccompanied by a single drop of bleeding, and none followed afterwards. At its conclusion the patient was in an extremely exhausted condition. She was put to bed, stimulants administered, and under proper feeding and tonics she made an astonishingly quick recovery, and remains perfectly well at the present time.

The tumour was as large as a bantam's egg, perfectly spherical in form, with a pedicle an inch in diameter, had several stellate cracks or fissures extending through the mucous membrane by which it was covered, which were undoubtedly the source of the repeated hemorrhages; in structure it was a firm fibroid, and resembled accurately, as it did also in its symptoms, the well-known fibroid polypi of the uterus.

Remarks.—Fibrous tumours of the tongue must be considered as very rare, and all the cases that I have been able to find recorded have occurred on the dorsum or anterior parts of the organ. I, therefore, regard this case, with regard to its situation at least, as *unique*.

In Holmes's *System of Surgery* fibrous tumours of the tongue are not noticed, and in the great majority of text-books and works on surgery, both old and new, no mention is made of any such affection. Under the head of fibro-cellular tumours Mr. Paget has the following:—

"A fifth is an oval bilobed tumour, about half an inch in diameter, which I removed from a young man's tongue, in the very substance of which, near its apex, it had been growing for three years. It was firmer than most of the

others, yet succulent, and formed an obscurely filamentous tissue, abundantly nucleated."—*Surgical Pathology*, p. 390.

In Cooper's *Surgical Dictionary*, under the article Tongue, Diseases of, there is an account of a circumscribed tumour of the middle of the tongue in a young man, but the account is so short and meagre that no opinion can be formed of its nature. Brodie (*Clinical Lectures on Surgery*, p. 151) speaks of a tumour of the tongue which was cured by the administration of tinct. iodini, but as he says *he* can offer no opinion as to its nature, and gives but a short account of it, we may be excused for not having any to offer.

Gross (*System of Surgery*, vol. II., p. 470) says:—

"Tumours of a fibrous or fibro-cellular structure are sometimes found in the tongue, either imbedded in its substance or attached to its surface. They are generally of a spherical or ovoidal form, hard and firm in their consistence, free from pain, and of a tardy, painless development. When they spring from the surface of the organ, they usually present a peculiar pendulous, polypoid appearance, their connection being effected by a narrow, slender pedicle. The fibrous tumour of the tongue is occasionally congenital."

This is all, and is the best account of the disease that I can find.

In the *Boston Med. and Surg. Journal*, of 1855, there is an account by Dr. A. Waterhouse, of Exeter, Maine, of the removal of a fibrous tumour of the tongue. The account is so interesting that I transcribe it entire.

"Wm. Canney, of Exeter, æt. 16 years, in good general health, came to me, with his father, wishing my advice in respect to a tumour situated on his tongue. The tumour was first noticed about three years previous, then of small size, but increasing gradually since, until the time of my seeing it, when, from its size, it caused him great inconvenience. His face was somewhat enlarged on the right side, in consequence of the enlargement of the buccal cavity to accommodate itself to the slow yet steady growth within. On examining the tongue I found a tumour of considerable size situated on the right side of its upper surface, at the same time inclined to the side, so as to push the substance of the organ to the left, and compress it to very narrow limits. The tumour was of considerable firmness, somewhat elastic, and very well defined anteriorly, at the distance of an inch from the extremity of the tongue, when the organ was in a state of rest, and not appearing to implicate its structure; but posteriorly it seemed to be combined with the substance of the tongue and not so well defined. Injected vessels of large size were running over its posterior surface and extending along its borders; otherwise the skin appeared to be healthy, though much stretched. It had caused him no pain, but within a few months there had been some soreness at its upper part. Different opinions had been expressed by physicians who had previously examined the case, relative to its character and probable termination.

"After a careful examination I diagnosed a fibrous tumour and prescribed removal, as the only means of relieving him from such a barrier to speech and deglutition, and also from the inevitable consequences should it thus be allowed to remain.

"Accordingly, on Jan. 23, 1854, chloroform having been administered, I proceeded to operate by first passing a strong ligature through the end of the tongue to secure its movements; I then directed an assistant to draw the tongue forwards and to the left, thereby bringing the tumour as far toward me as practicable, and plunging into it a hook, I made a semilunar incision along its side, and another along its superior border, including between the two all the integuments, except what was considered sufficient to close the wound. The anterior portion was easily separated from the parts beneath; but posteriorly

it was found to involve the substance of the tongue, so that I was obliged to carry the dissection deeply into the organ, in doing which the lingual artery was divided, and sprung furiously. The tumour was immediately removed, and the artery seized with a forceps. A ligature was applied and knotted by means of a thumb forceps in each hand; an operation obviously attended with some difficulty from the situation of the vessel, but which I preferred to other methods usually resorted to under like circumstances. After the hemorrhage had been arrested, the wound was cleansed and brought together by three points of interrupted suture, and the operation finished; the whole time occupied being only a few minutes. The wound healed kindly, so that in fifteen days after the operation he was able to attend his school and read aloud tolerably well. The tumour after removal was of an ovoid shape, two and a half inches in length, one and three-quarters in breadth, and weighed twenty-two pennyweights. Microscopic examination proved it to be of a fibrous structure, involving but not invading the muscles with which it came in contact.

"Slight traces of fat were found in its central portion, where it was of a brownish color, but nothing malignant could be detected about it."

The cure proved permanent.

In conclusion, I will just mention in addition to these cases of disease of the tongue, an example of congenital deformity of this organ which came under my observation Nov. 15, 1869. It was in a female infant, in whom the tongue was cleft or bifid at its end for about one inch, and had beside on its left side a large depression or sulcus at nearly the middle. The child was the subject of other deformities, and a full report of the case may be found in the *New York Med. Gazette*, April 30, 1870.

ART. X.—*Cases of Ovariotomy.* By WASHINGTON L. ATLEE, M.D., of Philadelphia. (Reported by J. EWING MEARS, M.D., of Phila.)

CASE 230. *Unilocular Ovarian Tumour; Pelvic and Intestinal Adhesions; Incision four inches long; Recovery.*—April 19, 1866, Mrs. M. C., of the vicinity of Princeton, N. J., consulted Dr. Atlee. She was 37 years old, and first menstruated at the age of sixteen, and was irregular until the age of twenty, having intervals sometimes of six, and at others of twelve months. She was married October, 1858, but never conceived. After the age of twenty menstruation became more regular, and after marriage more profuse and more frequent, occurring every two or three weeks. Her health has always been delicate, and she suffers from pain in the head, in the groins, and the left side. Has always had a poor appetite.

She varies in size, and for the last two years she has never been smaller than she is now, but often has been much larger. She, however, is as large as a woman at the full period of gestation. The abdominal tumour is uniform in shape and globular, elastic, semi-fluctuating, free from nodules and ridges, and quite smooth. The percussion sound is resonant in the upper portion of the abdomen, and dull elsewhere. The uterus is displaced to the right side of the pelvic cavity, and the sound enters it four and a half inches, ascending about three inches above the pubes, so that its end can be felt to the right of the linea alba.

Diagnosis.—A unilocular ovarian tumour, or a cyst of the broad ligament.

December 29, 1868, Dr. Atlee tapped the patient, removing twenty-three pints of very dark chocolate-coloured or deep-brown fluid, which looked almost black in the tub, and which coagulated partially by heat. After tapping, the *diagnosis* was unilocular ovarian tumour.

May 19, 1870, Mrs. C. called to see Dr. Atlee, and stated that for nearly a year after the tapping, she had remained free from any accumulation, before she began to fill again. She now, however, is as large as at the time she was tapped, and has decided upon ovariotomy.

September 21, 1870, Dr. Atlee visited her residence for the purpose of removing the tumor. Drs. Wikoff, Bartine, and Schenk, of Princeton; Professor Blaney, of Rush Medical College, Chicago; Dr. Barron, of New York city; and Dr. J. Ewing Mears, of Philadelphia, were present.

The operation was performed in the usual manner, the adhesions being separated, and the bleeding vessels secured by ligature. The tumour was composed of one large cyst, with a small multilocular mass in its wall, and involved the right ovary. The recovery of the patient was tedious, but in every respect satisfactory.

CASE 231. *Multilocular Ovarian Tumour; tapped six times; Inveterate Parietal Adhesions; Intestinal and Vesical Adhesions; Incision six inches long; Estimated Weight of Tumor,* 120 *lbs.; death in twenty-eight hours.*—August 10, 1870, Dr. Atlee visited Miss S. S. at Westport, Connecticut, in consultation with Dr. G. B. Bouton. She was thirty-five years old, very much emaciated, and had been tapped five times, losing at one time the enormous quantity of twelve gallons. The fluid at the first tapping was transparent, but was opaque at the subsequent tappings. After being tapped a very large solid mass could be seen and felt in the region of the liver. This mass was supposed by her physicians to be the liver in a state of hypertrophy, and adherent to the multiloclar tumour so intimately as to render the removal of the tumour impracticable. She was last tapped about four months ago. The enlargement commenced about three years ago.

The abdomen is enormous. She measures round the waist thirty-six inches; round the umbilicus, fifty; from sternum to umbilicus, seventeen; from sternum to pubes, twenty-eight and a half; between the anterior superior spinous processes of the ilia, thirty-three and a half. The emaciation extreme, the pulse feeble and very frequent; tongue rather red; appetite pretty good, but digestion feeble; bowels irritable, and defecation prostrates her. Menstruation has been suspended for seventeen months.

The abdomen is very wide, somewhat irregular, and covered by a network of very dark-coloured veins; in some places the blood in the veins seems to be coagulated. A large cicatrix exists at the point of tapping below the umbilicus. There is no resonance on percussion, except over the left lumbar region. Fluctuation is distinct over the whole left side, extending to the central and lower portions of the abdomen; but there is no fluctuation over the right side. There is some fluctuation over the right lumbar region, indicating an additional small cyst there, which does not communicate with the large cyst in the left side. The whole right side is evidently occupied by a multilocular mass, while the left side contains one large cyst. The portion, supposed by her physicians to be the liver, is evidently a part of the multilocular mass itself. The lower extremities are

œdematous. The pelvis is free, the cervix uteri normal and thrown back. The sound enters the uterus about two and a half inches, and the organ appears to be movable.

Tapping was performed with a long, large trocar, removing about five gallons of a purulent-like looking fluid, with cholesterine floating on it, and partially coagulable by heat. When collected in the tub the fluid closely resembled soft soap, having the same mottled appearance. Before the canula was removed the multilocular masses were punctured, and a little fluid resembling the white of egg, and some blood, were evacuated. The abdomen had now subsided over the left side, but a prominent tumour occupied the region of the liver, which was visible along the whole right side.

Diagnosis.—A multilocular ovarian tumour.

As an operation offered her the only hope of relief, Dr. Atlee told her that she was entitled to the chances which it afforded her.

A specimen of the fluid was handed to Dr. Mears, who reported it ovarian in character.

October 5, 1870, ovariotomy was performed, Drs. Bouton of Westport, M. B. Pardee of South Norwalk, Samuel Syne, and J. W. Barbour, of Norwalk, being present.

The tumour was found to be multilocular, and involved the right ovary. The tumour and its fluid contents were estimated to weigh 120 lbs.

The patient bore the operation pretty well. No sickness of stomach occurred. The pulse, which was feeble before the operation, lost force, and was very weak when the patient was placed in bed.

She passed that day and night in tolerable comfort, but sank gradually next day, and died about 3 P.M.

Remarks by Dr. Atlee.—The above was one of those desperate cases which we place under class second, where the operation offers the only chance in warding off impending death. The patient had been greatly reduced by repeated attacks of peritonitis, following the tappings, and the functions of life were much impaired thereby, as well as by the immense bulk of the tumour. It was unfortunate that the operation was delayed until the tumour had developed to such an enormous size, had formed such inveterate adhesions, and had, through inflammation and other causes, overcome the vital stamina.

ART. XI.—*Report of Two Cases of Inversion of the Uterus, with Remarks and a Description of the Uterine Repositor.* By JAMES P. WHITE, M.D., Professor of Obstetrics in the Medical Department of the University of Buffalo. (Read before the Medical Society of the State of New York, February 6, 1872.)

THE *Eighth Case* of inversion of the uterus, which has been treated by me, occurred near Ithaca, Tompkins County, New York.

The following is an extract from a letter received from Dr. H. B. Chase, of Jacksonville, who was then in attendance upon the patient:—

"The case is as follows: Mrs. R., æt. 27 years, of a good constitution, was delivered on the morning of the 25th January, of her first child, after a tedious labour of eighteen hours, when her medical attendant discovered that the placenta was adherent. Severe hemorrhage came on when the doctor separated the placenta from the walls of the uterus and withdrew it with his hand. Immediately (so he says) he discovered a large solid tumour in the vagina, accompanied by still more profuse hemorrhage.

"Four days after the accident I was called to see the patient. On examination I found the uterus completely inverted. The os uteri could be felt at the superior extremity of the tumour. The uterus, at that time, hard and unyielding and filling the vagina. I made an effort to reduce it, but failed. I felt quite sure that reduction was impossible at that time. And now, doctor, the question I would ask is: Can anything be done to restore this poor woman to health and usefulness again?"

My reply to Dr. Chase's inquiry held out the hope that it could, by suitable manipulation, be restored. Accordingly, in compliance with an invitation contained in a subsequent letter, I visited the patient on the 27th of February, not quite five weeks after the accident.

I found her very exsanguine and feeble, constantly losing blood, and at times bleeding profusely. The uterus had not completed its involution, though it was nearly as small and firm as in the normal condition of the organ in the multipara in its unimpregnated state. The perineum had been lacerated during the labour, and the hand could be easily introduced into the vagina and up to the neck of the uterus. The inversion was found to be complete, although the uterus did not protrude beyond the vulva.

The bowels had been, by my direction, freely evacuated the same morning, and, after drawing off the urine, I proceeded to reposit the organ, in the presence of Drs. Chase, Lewis, White, and Carrington, and Mr. George Rightmire, medical student, who has furnished a partial report of the case in the *Buffalo Medical Journal* for April, 1871. Requesting one of the gentlemen to take charge of the anæsthetic, whilst two others were seated beside the bed, each holding in his lap a foot, supporting with one hand the knee upon his side and holding firmly the patient's hand upon the same side, with her hips brought to the edge of the bed, I placed myself upon my knees and commenced the operation. Carrying my right hand into the vagina, I seized the entire organ and manipulated and compressed it firmly for a short time, to expel the blood and render the tissue flexible. This pressure was continued, with the hand within the vagina, during the entire process. I now commenced pushing upon the fundus, with a large rectal bougie, alternating with the thumb and fingers of the left hand and thus put the vagina upon the stretch. This process, through the attachment of the vagina to the inverted uterus, pulled open the os and reflected it upon the neck. Thus it will be perceived, as I have heretofore described, the process of restoration was from neck to fundus. The fundus was at no time perceptibly dimpled or doubled in upon itself. By continuing this process steadily and gently, occasionally placing the fingers of the left hand over the hypogastrium and through the thin walls of the abdomen, inserting them into the opening os and making traction and pressure, I found the organ passing up through the neck and assuming its normal position. At the expiration of twenty-three minutes from the time of commencing the operation, the uterus was completely restored. The loss of blood was small, and the only noteworthy peculiarity in the

case was the doughy *feel* of the organ in this stage of involution. It did not possess the firmness of normal uterine tissue in the unimpregnated state, nor the muscular flexibility of the recently delivered organ; but had a doughy feel like an organ undergoing fatty degeneration. Indeed I am induced to suspect that, at this period after delivery, the uterus cannot be subjected to manipulations without danger of laceration, which would be perfectly safe at a later period, after complete involution had taken place. It is the only case which has fallen under my observation between the third and the eighth week, and the sensations occasioned by pressure and other manipulations were such as to excite apprehension lest the tissues would yield under the fingers.

The patient soon recovered fron the effects of the anæsthetic, and I left her, a few hours after, sleeping comfortably under the influence of an anodyne. Since that time she has, under the care of her attending physician, Dr. Chase, gradually convalesced, and is now in "good health."

CASE IX.—The following history is taken from notes furnished by J. W. Stewart, M.D., the attending physician:—

"Mrs. E. A., of Port Dover, Canada, æt. 34, was delivered of her second child on the 7th of October, 1870. The case for the first seven days I had no personal observation of, but will give a brief history as I learned it from the patient herself and her friends who were attending her, the correctness of which I do not doubt.

"The labour was comparatively an easy one, lasting but three or four hours, and having no pains as usual for a few minutes after the birth of the child. The medical attendant, finding that the placenta was not expelled, made an examination, and said 'it was grown to the womb.' He caught hold of the cord, giving it two or three sharp jerks, and the after-birth came away. The patient, immediately after its expulsion, complained of violent and constant pain, and says she experienced a sensation as if 'all her insides had dropped down.' In a few minutes she began to sink, being bathed in cold perspiration, pulse running very rapidly. Her friends, being alarmed, asked the doctor what was the matter? He replied she was dying. After a time she rallied, but remained in excruciating agony up to the time I saw her—the eighth day after confinement. At my first visit I found her in the following condition: Tongue deeply furred; very irritable and feverish; pulse 155 to 170, very angular and weak; abdomen much swollen and so tender she could not bear the clothes to touch her. In a word, I found her labouring under a violent attack of metritis, the inflammatory action extending over the whole surface of the peritoneum. I learned that these symptoms had appeared three days previously, hourly growing more intense. I removed her bandage immediately, and applied turpentine stupes and warm fomentations, giving her large and repeated doses of opium.

"By my advice Dr. C. W. Coventer was called in the same evening and the next day. She described to us so accurately the symptoms of inversion of the uterus, taken in connection with the history of the case and her strange feelings, that we suspected the true state of affairs, and suggested an examination. This she positively refused, saying she 'was so weak and tender it would kill her,' and we were compelled to postpone it till the next day. The next morning I found her better, and the whole surface of the abdomen terribly blistered from the free use of turpentine. The inflammation and swelling were subsiding rapidly, and I insisted on making an examination; when I found the uterus inverted, and almost protruding from the vagina.

"Continued the warm fomentations, keeping her thoroughly under the influence of opium, with quinia, beef-tea, &c. &c.

"In a few days she improved wonderfully, and in two weeks we considered her strong enough to bear the operation, and attempted its reduction. We first brought her under the influence of chloroform, and then introduced the hand into the vagina, trying to reduce it by a kneading process with constant pres-

sure. We succeeded so far, after two hours' constant labour, as to get the fundus even with, if not a little inside of, the os. We tried to keep what we had gained by inserting and inflating gutta-percha balls, which we had procured for the purpose. Next day she appeared to suffer very little from the operation; but the India-rubber balls did not come up to our expectations, so the womb had dropped back into nearly into its former position.

"About eight weeks after, we made a second attempt, but the result was less favourable than the first. I forgot to mention that, about midway between the first and second operations, she had another violent attack of inflammation without apparent cause."

Upon receiving an invitation from Dr. Stewart, I visited the patient on the 11th of March, 1871, nearly six months after the inversion. I found the woman very prostrate and anæmic, and suffering from constant copious leucorrhœal discharges, and frequent hemorrhages. The uterus was completely inverted, with its fundus just within the vulva, and having fully undergone the process of involution. It was now as hard and firm as in the normal condition in the multiparæ. The bowels, by my direction, had been freely evacuated before my arrival, and I proceeded at once to make an effort to reposit it.

Placing her in the same position as heretofore described—upon the side of the bed, with her feet supported by Drs. Coventer and Salmon, I assumed my usual position upon my knees. Dr. Stewart, her family physician, was charged with the administration of the chloroform, and requested to produce profound anæsthesia. The first few minutes were chiefly occupied in compressing and moulding the uterus, rendering its walls more flexible, and in gently pressing up the organ so as to put the os upon the stretch and commence its dilatation. At this time the *uterine repositor* was introduced into the vagina, and its disk placed in contact with the fundus, and firmly held there by the intra-vaginal hand. The outer end of the instrument was placed against my breast, on the same level with the uterus. By means of the large circular spring, it readily kept its place, and left the other hand free to be used above the pubes to assist in forcing open the dilating os, which could be plainly felt through the abdominal walls. The spring at the outer end of the instrument enabled me, without danger of lacerating the tissues, to keep up a constant gentle pressure upon the fundus, and by leaning forward, to increase this pressure intermittingly. The force was exerted more directly upon the fundus by means of the repositor than was possible by the thumb or fingers, or by the round end of the large bougie. Pressure made through this spring is made more continuous than when made by the hand, and far less likely to lacerate the tissues. It was the first time I had used it, and I was delighted to find that it gave me a third hand, *which did not become fatigued*, and permitted me to use the left hand in manipulating the os in the hypogastrium; while the right hand easily held the instrument in contact with the fundus, and firmly grasped that part of the uterus not yet reflected, and remaining in the vagina. At the expiration of an hour and ten minutes, considerable progress had been made in reflecting the os down upon the body of the organ, when the patient seemed in some danger from the chloroform, and the process was for a short time suspended. The windows were thrown open, a little brandy was given by Dr. Hayes, who was present, and in a short time it was again deemed safe to resume the operation. Remaining continuously in the same position, I resumed pressure upon the repositor, holding, with my left hand, the anterior reflecting surface of the uterus which extended further and

further up into the abdomen. In one hour and twenty-three minutes from the time of taking my position, I had the pleasure of announcing that the organ was completely restored. As my associates had previously made much longer efforts, under circumstances much more favourable, they were at first incredulous, but soon verified my assertion by instituting, at my request, a careful examination.

Placing the patient comfortably in bed, and giving a full dose of morphia, she slept well that night, and I left her next morning feeling quite comfortable, and delighted with the prospect of being again restored to health and strength.

Nothing remarkable occurred during her convalescence, which, though slow, was uninterrupted, "riding out" during the month of April; and in the course of the summer, she regained, I understand from Dr. Stewart, her "usual health."

Remarks.—These two cases complete a series of nine cases of inversion, varying in duration from a few minutes to fifteen years after its occurrence, which have been reduced, by the writer, by manipulation at a single effort. The first case was reported to the Buffalo Medical Society in February, 1856, and was of only eight days' standing. After giving a full account of the case, which was published in the *Buffalo Medical Journal* of March, 1856, the writer concludes with the following remarks:—

"This case is regarded as interesting in many respects. It will encourage the growing belief among accoucheurs that reduction may be undertaken with reasonable hope of success, at a period much later than most writers have heretofore advised."

In the same article, alluding to a case of fourteen days' standing which had fallen under my observation in 1842, and in which no attempt at restoration was made, it is added: "With my present views upon this subject, I should abandon such a case as hopeless only after a long effort at reposition."

Fully convinced, from the result of the efforts made in this instance, eight days after inversion, of the feasibility of restoring the uterus in many cases heretofore considered irreducible, I did not meet with an opportunity of putting my convictions again to the test until the 12th of March, 1858, when I visited Mrs. M., near Hornellsville, who had been confined on the 22d of September previous, almost six months before; and after about one hour's continuous effort, succeeded in repositing the inverted organ. A full account of this case, with plates, and a description of the manner in which reduction was accomplished, was published in the July number of the *American Journal of the Medical Sciences*, for 1858. Permit me here to crave indulgence for one word in relation to priority in this operation. It is true, that Tyler Smith published his case April 24th, 1858, in the *London Medical Times and Gazette*, forty-two days after my operation in Hornellsville, and more than two months before the report of the case made its appearance, in the *American Journal of the Medical Sciences*. But I submit, that, more than two years

before, I had taken the initiative, and published my views and hopes. And I also insist that his subsequent success in April, 1858, could hardly have been known in this country on the 12th of March preceding, which is the date of my second operation. Upon this point I trust I shall be pardoned for gratefully quoting the following passage from the able article on this subject from the impartial and distinguished Professor of Obstetrics in the University of Louisville.

"Before I proceed to do this [give an account of the method of manipulation pursued by Dr. White], it is meet that I should pay a tribute of honour to our countryman, Professor James P. White, who has not received the credit he so well deserves, for his leadership in the revolution of gynæcological practice, which he inaugurated. Even a superficial reading of his report[1] must satisfy any one that he regarded himself as a pioneer in his attempt to reduce, by the taxis, chronic inversion of the uterus; and that he was wholly unconscious that such a surgical feat had been performed either at home or abroad. Neither had it been so much as thought of at home, much less performed, nor had it been performed abroad in such a way as to attract professional attention or inspire confidence in it, until Dr. Tyler Smith, of London, published a case of twelve years' duration in the *Medical Times and Gazette*, April 24, 1858, which of course could not have been known to Dr. White, whose case was successfully treated on the 12th of March immediately preceding, though not published until July following, the earliest time that it could be published in a quarterly journal."[2]

On the 24th of August following, in the presence of Professors Austin Flint, Sen. and Jr., and Professors Thomas F. Rochester and Mason, I reduced an inverted uterus of over fifteen years' duration. This was accomplished in about fifty minutes and with less difficulty than those of six months. It should be stated, that the patient died on the sixteenth day after the operation, though it does not follow that her death was a consequence fairly chargeable to it.

An account of this case, as reported to the Buffalo Medical Association, will be found in the *Buffalo Medical Journal* for October, 1858, and from which the following paragraphs are cited:—

"She improved rapidly, with no untoward symptoms during the succeeding eight days, and at the end of that time considered herself perfectly well. On the morning of the ninth day, however, after returning from breakfast, she imprudently strained considerably at stool, and was suddenly seized with violent pains in the abdomen. She was at the same time startled by the coming in of a friend whilst she was on the vessel. The mother and husband had left her a day or two before, considering her perfectly well. She immediately went to bed, sent for me, and died on the seventh day after of peritonitis."

In commenting upon the report of this case in the Association, Professor Austin Flint, Sen., holds the following language:—

[1] Alluding, doubtless, to the case of 1858, and never having had his attention called to the one of January, 1856, reported in the Buffalo Journal more than two years before.

[2] Thoughts on Chronic Inversion of the Uterus, specially with reference to Gastrotomy, as a substitute for Amputation of the Uterus. By Henry Miller, M.D. Richmond and Louisville Medical Journal, April, 1870, p. 14.

"I was present at the reduction of the uterus by Professor White, and also at the post-mortem examination. The reduction was effected with great ease, and under the moderate use of chloroform. He (Professor Flint) regrets, equally with Dr. White, the unfortunate accident which resulted in the death of the patient. The connection between the peritonitis and the operation did not seem to him to be very close; otherwise it would have supervened sooner. He did not think that it at all invalidates the success or propriety of the operation. The result will, perhaps, be thought to have more connection with the operation from an ordinary perusal of the case, than really exists. But a careful review of all the circumstances seems to show that the peritonitis was merely an unfortunate accident," &c. &c.

Professor Rochester said, "there were some points which had not been sufficiently dwelt upon by Dr. White or Dr. Flint. The appearance of the uterus, on examination after death, plainly indicated that no undue amount of force had been used in the operation. . . . In regard to the occurrence of peritonitis, he thought that it was a most unfortunate accident. Three-fourths of the cases which he had seen occurred at this season of the year. There was no doubt but that the exhausting nature of the difficulty under which the patient had laboured for so long, made the system more susceptible to the disease, and much less able to resist it. There was nothing in the appearance of the parts which would lead to the suspicion that the peritonitis commenced on the uterus; that it appeared to be a simple case of the disease. The greatest pain was referred to the epigastric region, and it was probable that this was its origin."

This case, of fifteen years, was much more easily reduced than the ninth case, just reported, and I am convinced that the opinion long since published by the writer that the difficulty of reduction is "as great immediately after complete involution, as at any subsequent period, however remote," will be found to be correct. I fail also to perceive why the *dangers* should be greater in consequence of delay, and hence my anxiety to prevent any erroneous conclusions upon that point which might be hastily drawn from the fact that this case terminated fatally, whilst those of six months recovered. The uterus, it is well established, completes the process of involution in from eight to twelve weeks. From the latter period onward I can perceive no cause arising from changes in the tissues of the uterus or surrounding organs which would enhance the difficulty or danger of reduction. At twenty years I should expect no greater difficulty in reduction than at the end of the same number of weeks. Judging from the cases of eight days, twenty-one days, and thirty-five days, and the eighth case just reported, it is, in my opinion, far more likely that it will be found by careful subsequent observation that the tissues are more susceptible to laceration from the necessary manipulations, during the latter part of the period of involution, than at any other time. As intimated in the report of this, the eighth case, which was between five and six weeks after labour, the impression in handling the uterus was that it would be easily torn. It is, during this change, neither muscular and flexible like the recently delivered uterus; nor firm and elastic like the unimpregnated organ. It will, I suspect, be found safer, in view of this condition, after about the twentieth day, to wait for the completion of this process,

notwithstanding the increased difficulties of the operation occasioned by such delay.[1]

Observation and careful reflection—based upon the cases which have been committed to my charge, now numbering nine in all, varying, as already stated, in duration from a few minutes to fifteen years—confirm me in the opinion expressed in 1858. Indeed, my convictions are very decided, and I am incredulous as to the necessity of ever resorting to amputation, or the still more objectionable operation of gastrotomy. True, it may not always be reduced at a single sitting, as in all which I have encountered, but by means of the repositor, uniform and gentle pressure can be continued until the os is fully dilated, and the fundus pushed up through it. The insurmountable difficulty heretofore has been supposed to consist in our inability to maintain uniform and persistent pressure for a sufficient length of time. The hand soon became fatigued, and another hand, even of the same individual, could not be substituted without losing a part of what had been gained. This loss was increased when the hand of a fellow practitioner was introduced to continue the operation. No doubt great physical endurance, being able to maintain one position for a great length of time, has been an essential element in success. The various substitutes which have heretofore been resorted to for continuing pressure, where the operator became exhausted, have utterly failed. The elastic bags, mentioned in the last case, used by Dr. Stewart, are a fair illustration of their inefficiency. They press more upon the large surfaces anteriorly and posteriorly situated, and with which they are in contact, than they do upon the fundus, which has no firm ossific base of support, as have the rectum and bladder. The uterus ascends very soon, owing to the yielding nature of the vagina, and escapes from the reach of the distended vaginal bags.

The repositor, when it is deemed better to proceed in a more gradual manner, or when it may be found impossible to reduce it at a single effort, can be received with the uterus into a large cylindrical speculum, and by means of a T-bandage, can be made to press directly upon the fundus until the os is gradually dilated and all resistance overcome. By means of the large spring at the outer extremity the amount of pressure can be graduated to an ounce. The disk will follow up the fundus by means of this continuous elastic pressure until it disappears in the os and neck. Any intelligent assistant can be trusted to increase or diminish the pres-

[1] Pages 102 and 103, Lectures by Priestley, on the subject of the involution of the uterine parietes, says: "With the advance of the fatty transformations the uterus becomes, in a corresponding degree, friable, and continues so until it has completely returned to its usual condition. I have occasionally seen at the post-mortem examinations of women, who had previously borne children, the uterine tissue affected by fatty degeneration, and so soft and friable that a sound passed into the uterine cavity might be readily pushed through the uterine walls.

sure during the absence of the practitioner, as the exigencies of the case may demand. The pressure which the repositor can be made to exert is equally as gentle and far more persistent than the tent of sponge or sea-tangle, or the wedges of Barnes, when applied to dilate the os for the purpose of examining the cavity, when the uterus is in its normal position. These are the views expressed fourteen years since, in the article in the *American Journal of the Medical Sciences*, already alluded to. The following passage occurs:—

"We are all aware that the os and neck of the uterus may, by sponge tents and other mechanical contrivances, be widely dilated when the womb is in its natural position and of ordinary size. This dilatation is frequently made by those who are extensively engaged in the treatment of uterine affections for the purpose of perfecting diagnosis or instituting treatment. Of the susceptibility of these parts to be thus dilated, and that, too, without much risk of injury to the tissues involved or the general health, no one at all familiar with the subject will deny. What is the relation of parts in inversio uteri? Is not the vaginal sheath, which in the normal arrangement of parts was attached to the outside of the neck of the uterus, now, in the inverted state, firmly attached *within* the cavity of the canal of the neck of the uterus just at its orifice? The vaginal canal is securely attached at its lower extremity at or near the outlet of the pelvis, and, whilst it is very elastic, is less yielding in its longitudinal than transverse diameter, and not easily lacerated or detached from its connections, unless irregularly pressed as by a pointed instrument. Force or pressure, applied to the fundus of the inverted uterus, is resisted by the upper extremity of the vagina, which is now fastened upon the inside of the neck. The lower extremity of the vagina, being firmly attached, cannot yield, and the inevitable result must be the pulling open the mouth of the uterus, unless the tissues are lacerated before that part dilates. Can there be any greater difficulty or danger in *pulling* open this outlet than in *pressing* it open if performed with the same gentleness?

"The uterus and vagina in complete inversion represent a continuous bag or sac, doubled or reflected upon itself, with the open extremity of the sac securely fixed. Pressure upon the closed end of the bag will, it is plainly perceived, under such circumstances, result in straightening the bag by completely turning it the other side out. So with the parts concerned in inversion. Pressure upon the fundus, if well directed, pulls open, first its mouth, then its neck, and finally, if persevered in, doubles the body upon itself also, and carries the fundus through the os and neck and body to its normal position.

"Does any uterine pathologist believe it would be impossible safely to dilate the os and bring down the fundus of the uterus—completely inverting the organ—if carefully and perseveringly undertaken? If affirmatively answered, then why may we not pull open the neck by means of the vagina in the same gentle manner as we would press it open when in a normal position, and thus carry the fundus up through it by means of pressure upon that part when it is in a downward direction? Perhaps I may be too sanguine, but I am inclined to believe that well-directed pressure upon the fundus, if continued long enough, will in all cases, where there are no adhesions, result in restoration or reposition, no matter how much time may have elapsed since inversion occurred."

Subsequent experience has but served to confirm the views then advanced. With the repositor, as before described, pressure is brought directly upon the fundus and maintained for any length of time desired and with such an amount of force as shall be deemed entirely safe. The uterus can only escape in an upward direction, and restoration must follow. Nor will it be more painful when thus dilated than when the os is dilated

by tents. I am of opinion that all cases of inversion *can* be reduced, without qualification as to their duration.

This opinion does not, however, seem to have been generally adopted by writers upon the subject, since these views were published, and hence it is deemed important to urge them upon the consideration of gynæcologists.

It is true, there are a few exceptions to this lack of faith. In an able "Report on Inversion of the Uterus," made to the New York State Medical Society, by appointment by Prof. J. V. P. Quackenbush, at its annual meeting in February, 1859, we find the following emphatic endorsement of the feasibility of reduction. After giving a very clear account of his theory of the causes of this accident, he says:—

"But whatever may be the manner of its occurrence; the proper treatment is what most demands our attention. And what is the proper treatment of inversion of the uterus? I answer, reduction. If the case be recent, reduction; if of twelve days' duration, reduction; if of twelve years' duration, reduction! At any time, and under all circumstances, reduction must be resorted to, and very few cases will be found which are irreducible. I lay much stress upon this subject, for I do not think it well understood." . . . Again, after referring to the opinion of Meigs when he says, "you have no art or skill or no power equal to the performance of such a miracle of surgery as that" (the reduction of a case of inversion of six months' duration), Prof. Q. adds, "gentlemen, this miracle has been done, and can be done, and should be done, and with proper management no case need be abandoned."

Notwithstanding this unhesitating affirmation of the doctrine of the feasibility of reduction in all cases of chronic inversion, which I had enunciated nearly two years previously, we find men in this state holding to the old doctrine that reduction is exceptional, and not by any means to be expected. The late Prof. Bedford, in his *Principles and Practice of Obstetrics*, Fourth Edition, 1868, uses the following language:—

"Should every effort *fail*—and such, in the most skilful hands, will not *unfrequently* be the case—care should be taken to return, if possible, the tumour within the vagina, and retain it *in situ* by the India-rubber pessary," &c. &c. &c.

Nor is this incredulity, or want of progress, confined to this country. In England, where Tyler Smith had reduced a case of twelve years' standing, and published it in April, 1858, expressing the opinion "that no case, with proper and prompt management, could be considered irreducible within a reasonable time after the accident," we find Dr. Meadows, in his *Manual of Midwifery*, published very recently, using the following language: "When the case has gone on unrelieved for many weeks, or even years, though attempts should always be made to secure reduction, &c. &c., yet the chances are *very greatly against success*," &c. &c.

Again, Courty, in his work published in 1866, in Paris, and who may be considered a fair exponent of the views in his own country, refers to reduction as being exceedingly doubtful. Indeed, he describes a method of reduction of his own which his reviewer, in the *American Journal of the Medical Sciences*, thinks "especially worthy of consideration and trial."

This consists in passing two fingers into the rectum and by depressing them into the anterior wall of the rectum, hooking them into the ligament on each side of the neck, and thus holding it down whilst, with the other hand, pressure is made upon the inverted portion of the organ, &c. &c. Again, he adds: "If the reduction is impossible, it is necessary to resort to palliative treatment or to *extirpation* of the uterus for its radical cure."[1] Nor are our German friends more hopeful. In a work by Dillenberger, republished in London, in 1871, he says: "Chronic inversion is held to require palliation; if that is vain, removal by ligature or excision."

Whence this want of confidence? The cases of reduction are now so numerous, and the operation of such demonstrable character, as not to admit of doubt.

I am utterly unable to account for this unbelief. Mere duration, I must repeat, cannot militate against success after the first three or four months have elapsed. And I am entirely incredulous as to there being such adhesions between the walls of the uterus as to withstand long-continued gentle pressure. Force may be as gradually applied by means of the repositor and speculum, as the operator may deem necessary for the safety of the patient. When the os has been thus carefully dilated, the hand of the operator, or, if the fundus has passed up into the neck, a large rectal bougie, can be called in requisition to complete the operation. Indeed, I indulge the hope that the day is not distant when all cases of inversion of the uterus will be regarded by all intelligent practitioners as amenable to treatment.

Perhaps something should be said here in relation to the manner in which reduction is accomplished. There seems great and unnecessary ambiguity and confusion on this subject. In all recent cases the fundus can be pressed into the body and neck, or "dimpled," as it is termed, by pressure upon the most depending part. In this manner, I am certain, the two cases which were reposited immediately after delivery and, to some extent, the one of eight days' duration, were carried up. In the case of Dr. Lockwood, in 1861, the womb had been inverted for about forty minutes when I arrived. Administering some paregoric and brandy to restore her from the collapse, I seized the uterus, pulled off the partly adherent placenta, and passed my hand, with the fundus before it, up into the cavity of the abdomen with scarcely more difficulty than would be encountered in inverting a wet bladder. Retaining my hand there for a short time, contraction soon came on under the influence of the ergot and stimulants which were given, and the patient (Mrs. W.) made a good recovery, and has since borne children. So with the one at eight days; in the report, it it stated, "having succeeded in 'dimpling' the

[1] Page 80, Paris edition, 1866.

fundus, pressure was maintained with the thumb at that point until the hand became nearly powerless. To preserve this depression whilst the muscles of the hand were permitted to rest, a rectal bougie, about twelve inches in length and one in diameter, was carried along in its place, fixed in the dimple, and pressure unintermittingly continued through it, by the left hand outside the vulva. Gradually the concavity of the fundus was found to be deeper and deeper until it finally became completely restored." This is doubtless the manner in which reduction takes place in all recent cases, whilst the organ is large and the walls flexible, and I was led into error by it in supposing that this same method obtained in all cases. This delusion was dispelled in my next case, of six months, in the report of which it is said in relation to the manner in which reduction was affected: "There can be no doubt that the os first commenced to yield and pressed down upon the intra-vaginal hand, which, it will be recollected, inclosed the entire uterus and the upper extremity of the bougie and kept them in contact. This part gradually dilated and passed down upon and over the neck, which in turn dilated, and doubled down upon itself. The fundus did not perceptibly dimple, or was not reflected upon itself during the operation. The organ was too firm and the cavity too small for any depression to be made upon the walls of the fundus."

I have nothing to change in this description of the *modus operandi* of reposition in chronic inversion. In the case of fifteen years the attention of the distinguished gentlemen who were present was called to this fact (the reflection of the os upon the neck), and they were requested from time to time to pass a finger up beside the hand of the operator and verify this point. The same careful observation was made during the most difficult of all the cases of reduction which I have encountered, the one at Port Dover, and with the same result. In my opinion, it would be unwise to double in the fundus upon the body after complete involution, even if it were practicable to do so. By so doing you would increase the size of the tumour to be carried through the cavity of the neck and os, by this intro-flexion, and also increase the difficulties of reduction. But all who make the effort to dimple the fundus of the unimpregnated uterus into the cavity, will perceive that it is impossible to effect reduction or inversion in this manner, even upon the dead subject.

By this explanation can all the apparent discrepancies as to the manner of reduction be reconciled. One practitioner has operated upon a recent case and restored it by dimpling in the fundus, whilst his no less observing and truthful fellow has restored the involuted organ by reflecting the os over the neck and body without at all being able to depress the fundus, and hence the difference.

Never having had a case of inversion occur in my own practice, I have never witnessed it, and shall not occupy space with any theory as to the cause and manner of its production.

The following figures represent the uterine repositor and its mode of application :—

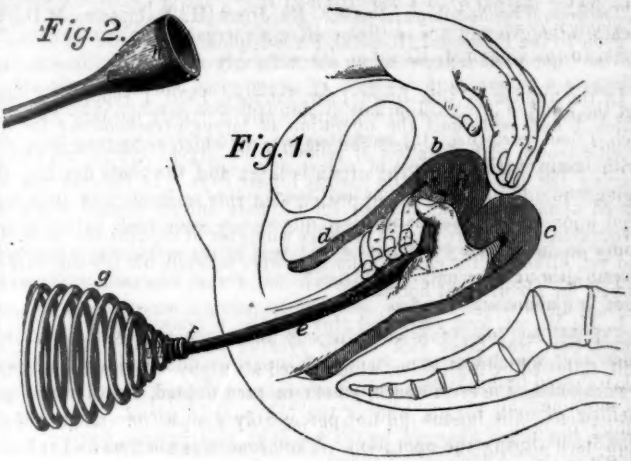


Fig. 1.

a. Uterus in process of reduction.

b. Anterior lip or wall of the uterus with the fingers of the left hand pressing upon it and assisting in pulling open the uterine cavity.

c. Posterior uterine wall semi-reflected.

d. Anterior vaginal wall.

e. Wooden or hard rubber stem of Repositor, its enlarged extremity held in contact with the fundus by the intra-vaginal hand of the operator.

f. Distal extremity of stem made into a screw, so as to fasten into *g*, a coil of No. 11 steel spring wire, requiring eight or ten pounds pressure by the breast of the operator, against which it is placed, to bring it down.

Fig. 2.

h. Uterine extremity of stem *e*, which is terminated with a soft india rubber disc 1¾-inch diameter, the concavity into which the fundus is received being about one-half inch deep, with its terminal margin thin and soft.

ART. XII.—*Report of a Case of Tonsillitis Associated with Parotitis; Laryngo-Tracheotomy; Death.* By JOHN H. PACKARD, M.D., Surgeon to the Episcopal Hospital, Philadelphia.

IN the number of this Journal for January, 1870, I reported a case in which I had performed the operation of laryngo-tracheotomy for diphtheritic croup, with the effect of relieving the urgent symptoms, and prolonging the life of the little patient for six days. In the case now to be placed upon record, the chances for ultimate success seemed much more favourable, since there was no evidence whatever of diphtheritic poisoning; and yet, although the child's sufferings were relieved, his life was only prolonged for about eight hours.

L. H. M., æt. four years and nine months, a remarkably handsome and robust boy, was taken sick on the evening of February 17, 1872, with chill, followed by high fever, which lasted all night. When I saw him the next morning, the fever had in great measure subsided, but there was great swelling of both tonsils, and of the left parotid gland. His voice was strong, but much changed by the tonsillar enlargement, having what is known as the "Punchinello" sound.

I applied a solution of nitrate of silver, grs. xx. to the ℥j of water, to the tonsils, and put him at once upon the internal use of quinia and iron, with beef tea and milk, to which brandy was afterwards added, as, from the difficulty with which he swallowed even liquids, he soon showed signs of debility. Some relief was gained by the application of hot poultices, and of raw cotton heated, over the swollen parotid. The air of the room was kept at a temperature as nearly 72° Fahr. as possible, and moistened by the vapor from a vessel of hot water.

No material change took place, however, until the evening of February 19th, nearly forty-eight hours after the onset of the disease. He was then slightly cyanosed from the obstruction to his breathing, and I immediately sent for Dr. Hutchinson, who, at my request, had seen him with me earlier in the day, and for Dr. Brinton. The difficulty of respiration becoming urgent, both gentlemen agreed with me that the only resort was the performance of laryngo-tracheotomy.

Accordingly, at 9 P.M., chloroform was administered by Dr. T. E. Ridgway, and I proceeded to operate. The swelling on the left side added somewhat to the difficulty of the case, but there was no bleeding, and the cricoid cartilage and upper rings of the trachea were readily reached. Just at the moment when I enlarged, with a probe-pointed bistoury, the opening into the trachea, there was a rather free gush of venous blood. The ordinary double tube was at once inserted, and after the expulsion through it of a quantity, not very large, of blood, breathing was, with some trouble, established. Artificial respiration by Silvester's method was of much avail in doing this.

The child's temperature and pulse now became very much improved, and eventually normal. Some excitement ensued as the effects of the chloroform passed off, but in an hour or two a quiet and healthy sleep came on, only broken by the occasional clogging of the tube with small quantities of blood, easily swabbed away. He sometimes asked for bits of ice, or for ice

water, which comforted him greatly, but he would not take brandy, or nourishment in any form.

At about 6 A.M., somewhat more than eight hours from the completion of the operation, I was about going home, when I noticed a change for the worse in his face, and in the temperature of his hands, which were growing cool. A few minutes after, his pulse became imperceptible, and in spite of stimulation by enemata, by external heat, and by a magneto-electric battery, he sank, and died quietly at 7 A.M.

No post-mortem examination was permitted.

Within the past week I have seen several other cases of this combination of parotitis with tonsillar enlargement, but none in which any grave symptoms presented themselves, except the one now related.

Perhaps it should be stated that the propriety of scarifying the tonsils was carefully discussed before the operation of tracheotomy was decided upon, but the extreme urgency of the case, and the uncertainty of relief either from the former procedure or from cutting off the projecting portions of the glands, and the great danger of hemorrhage which it would be difficult or impossible to control, seemed to us sufficient arguments against such a course.

And notwithstanding the sad fact that the operation resorted to failed in the great object of saving the child's life, we cannot forget that by it the horrors of a death by suffocation were averted, and our little patient's last hours were at least painless. As to the actual cause of death, it seems to me that it may have been from the entrance of blood into the windpipe alongside of the tube; but against this it was impossible to guard further than we did, under the circumstances. It is greatly to be regretted that no autopsy could be obtained.

1928 Spruce St., Feb. 23, 1872.

ART. XIII.—*An Improved Strabismus Hook.* By SAMUEL THEOBALD, M.D., of Baltimore, Md. (with a wood-cut).

As the operation for strabismus was originally, and for a number of years thereafter, performed, it mattered little what form of hook was used for securing the tendon, upon which the tenotomy was to be performed. The conjunctiva and subconjunctival tissue were extensively divided over the insertion of the muscle, and the tendon being exposed was secured by the hook (or as is still practised in Vienna, by a pair of fixation forceps), and divided with scissors.

There were, however, several objections to this operation, and at present the "Subconjunctival Tenotomy" of Critchett, has almost universally supplanted it in England and I believe in this country as well. In Critchett's operation a comparatively small opening is made through the conjunctiva and the capsule of Tenon, at a point corresponding with the lower border

of the insertion of the muscle. Through this the hook is passed, and the tendon, being caught up by it, is divided subconjunctivally with probe-pointed scissors.

There is, however, one imperfection in this operation as hitherto performed. The tendon invariably slips off the hook before it has been completely divided. This is caused by the action of the scissors and may occur several times during a single operation, rendering necessary, each time, further excursions with the hook, before the tendon is divided to the satisfaction of the operator. Having seen this operation performed repeatedly within the last eighteen months at the various eye clinics of Europe and by the hands of the most skilful ophthalmic surgeons, I have no hesitation in saying that this accident is unavoidable with the strabismus hooks now in use.

It is for the purpose of remedying this defect of Critchett's operation that I have designed the Crochet Hook which is represented in the accompanying cut. With the exception of the crochet point it is similar to Von

a. Crochet hook, natural size. *b*. Magnified view of crochet point.

Graefe's strabismus hook. The tendon being secured by it, it is not necessary to force the point up against the conjunctiva, as is ordinarily done, so as to throw the tendon into the angle of the hook while it is being divided, but simply to hold the handle at right-angles to the muscle. This is an important advantage, as it greatly diminishes the laceration and stretching of the subconjunctival cellular tissue.

The operator, having ascertained that he has passed the point of the hook beyond the upper border of the tendon, and holding the instrument in the manner just described, may, with a few snips of the scissors, divide cleanly the whole tendon, without the danger of any of the fibres slipping off, and then the hook may be withdrawn without difficulty. That it really possesses the advantages which I have claimed for it, I have the testimony of such men as Bader, of Guy's Hospital, and Streatfeild and Lawson, of Moorfields Ophthalmic Hospital, London, who, having operated with it, are prepared to judge correctly of its comparative merits.

It is hardly necessary to add that the instrument must be carefully made so as to avoid anything approaching a fish-hook action. To do this it is only necessary that the receding point of the hook shall be smooth and rounded, and that the angle formed by its recession shall not be too acute.

BALTIMORE, March, 1872.

TRANSACTIONS OF SOCIETIES.

Art. XIV.—*Summary of the Transactions of the College of Physicians of Philadelphia.*

1871. *Dec.* 20. *On the Diagnosis of Anæmic Murmurs.*—Dr. J. H. Hutchinson read the following paper on this subject:—

Although much has been written upon the subject of anæmia, and upon the means of distinguishing murmurs accompanying it from those produced by cardiac lesions, yet cases occasionally occur in the practice of every physician, in which it is almost if not quite impossible to determine from a single examination whether or not there exists organic disease of the heart. Such a difficulty constantly arises in cases of acute rheumatism, a disease in which there is a decided tendency on the one hand to the occurrence of inflammation of the membranes of the heart, and, on the other, especially if the case have been treated by antiphlogistic remedies, or by large doses of alkalies, to the production of anæmia, which may persist long after the patient is well enough to leave his bed, or even the house, and which may, in consequence of the existence of cardiac murmur, give rise to the impression that permanent injury has been inflicted upon the heart. It, therefore, frequently becomes a matter of some importance to be able to say whether the murmur indicates valvular trouble, or whether it is simply an attendant upon alterations in the composition of the blood. In spite, too, of all that has been written, the cause of these anæmic murmurs still remains undiscovered. The definition of the anæmic murmur given by Walshe, the general accuracy of whose description will scarcely be impugned, is as follows: "An intracardiac hæmic murmur of this variety is of moderate or very slight intensity, commonly of medium or low pitch, short or moderately prolonged, of whiffing quality, very easily rendered temporarily harsh by excitement of the heart, and *modified in intensity by certain changes in posture.*[1] This murmur is, as far as I have observed, invariably basic in seat and systolic in time, produced at the orifices of the aorta and of the pulmonary artery—with a force at each, proportional to the power of its communicating ventricle; scarcely conducted along the aorta at all, frequently audible, on the contrary, at the second left or pulmonary cartilage; only in exceptional cases audible below the nipple; and never within my experience perceptible as far as the left apex." He also attaches much importance to the fact that cardiac murmurs of this class are generally accompanied by the venous hum. In regard to the mechanism of these murmurs he says this is obscure enough, but that it seems impossible to ignore the influence of the composition of the blood "in highly marked spanæmia;" he says, "neither the flaccidity of the veins, pressure on their surface, nor even velocity of current is required for the generation of murmur." Other writers are disposed to attribute their production to augmented friction of the blood against the orifices of the aorta and pulmonary artery, and others to the diminished tension of the valves and

[1] The italics are our own.

vascular walls, which is caused by anæmia, and which permits vibrations in them to be more readily excited and maintained than in health. All, however, so far as I am aware, with a single exception, are agreed that the seat of their production is the aortic or pulmonary semilunar valve. A paper will be found in the *Archives Générales* for 1866, in which the writer, M. Parrot, attempts to prove that they are really due to tricuspid regurgitation. This regurgitation is caused by an enlargement of the right auriculo-ventricular orifice, consequent upon dilatation of the cavity of the right ventricle. The walls of the right ventricle are thin in comparison with those of the left, and it is thought that when badly nourished, as they must be in anæmia, they oppose but a feeble resistance to the dilating influence of the blood. M. Parrot says that the so-called anæmic murmur is not heard immediately after a large hemorrhage, but sometime later, when the dilatation of the cavity has been produced. He says, moreover, that the seat of the greatest intensity of these murmurs is not, as has been generally supposed, at the pulmonary cartilage, but invariably over the body of the right ventricle, and generally at the level of the fourth rib, a little to the left of the sternum, and that they are constantly accompanied by pulsation of the jugular veins, which may be visible under ordinary circumstances, or may be only perceived when the patient is placed in bed with his head a little lower than his shoulders. It cannot be doubted that a murmur depending upon functional rather than organic disease is occasionally heard in the position above indicated, but there are few physicians accustomed to the careful examination of the heart, who will agree with M. Parrot that this is the most frequent seat of anæmic murmurs, or that these are constantly or even often attended by venous pulsation.

With the view of bringing to the notice of the profession a sign which I conceive to be of some importance in the diagnosis of anæmia, and which, if not wholly unknown, is at least practically ignored, I have determined to report the following cases, which are a few of those in which it has been observed.

CASE I. Annie C., æt. 18, born in Ireland, single, a domestic, admitted into the Pennsylvania Hospital, Nov. 17, 1870. Family history shows a tendency to phthisis. Has lost her mother and one sister of consumption. Her health has generally been very good; when well, is stout and strong and has a good colour. Came to America at the age of 15, and shortly afterwards began to "live out." Has lived in the same place during the past three years. Her work was hard, and she had sufficient though not very attractive food, and as a rule scarcely ever went out. She has never had malarial fever or rheumatism.

Began to menstruate when between 14 and 15 years of age, and the discharge continued at intervals of a month for a short time, but soon afterwards recurred regularly every fortnight, and has since continued to do so, when well. The discharge continued for from two to three days, and was usually abundant, necessitating frequent changes of napkins. Two years ago she became without any apparent (additional) cause, very pale and weak, and her menses ceased entirely for two months. She lost flesh and strength, and was obliged to give up work. Took tincture of chloride of iron, and went into the country. This treatment soon restored her to health, and her menses reappeared. Last July, after unusually close confinement to the house and hard work, she lost appetite, became pale, felt weak, grew thin; had shortness of breath and palpitation on slight exertion, and her menses appeared only once a month,

while their duration was a little extended. At this time she had frequent bleeding at the nose. Has never had cough, never hæmoptysis, and never any uncommon loss of blood save by the frequent menstruation. She took tincture of the chloride of iron without apparent benefit. She continued to lose colour until admission.

Two weeks ago began to suffer from severe irregular pains, neuralgic in character, at the base of the chest on both sides anteriorly. Has never had leucorrhœa.

On admission she was markedly anæmic; the hands and face were white, the circulation of the surface sluggish, the lips and tongue pale, and the conjunctiva pearly, tongue clean, appetite poor, and her bowels constipated. She has no gastric or pulmonary symptoms. She sleeps well.

The urine is somewhat increased in quantity, but otherwise normal. Over the base of heart is heard a well-marked anæmic murmur. It is systolic in time, soft in quality, and is not heard at the apex. When the patient is placed in the recumbent posture, this murmur becomes much more intense, and this is evidently not due to increased action of the heart. The murmur becomes less intense when the patient again assumes the erect position. There is no diastolic murmur, and no reason to suspect any organic disease of the heart. At the sides of the neck, between the two insertions of the sterno-cleido mastoid muscle on the right side, is heard a continuous, musical, venous hum.

Dec. 23. Patient improved very much, her gums, which were so pale, have now a good colour, the colour has returned to her lips and cheeks; a soft murmur is still heard at the base of the heart.

Jan. 1, 1871. Condition still continues to improve, the soft murmur before remarked has never disappeared.

16*th*. Discharged, much relieved.

CASE II. John P., æt. 16, born in England; occupation, sailor boy, admitted April 13, 1871. Family healthy; father died of a fever; previous health has always been good, and has been a sailor for about 17 months. Was taken sick on voyage from the West Indies; had fever,(?) general weakness, &c. Soon after his feet swelled, and he became unable to attend to his duties.

13*th*. Present condition anæmic and weak, feet and legs swollen and pit slightly on pressure; appetite good; no symptoms of digestive trouble. A soft systolic murmur is heard at the pulmonary cartilage; by placing the stethoscope over either jugular, the "bruit de diable" can be detected. Urine was examined, but neither casts nor albumen were found. Passes urine in normal quantity; symptoms seem to be dependent upon the anæmia. He was ordered the infusion of scoparium and the citrate of iron and quinia in doses of five grains three times daily, and good diet.

19*th*. Œdema diminished considerably since admission. The infusion of scoparium was only given for a few days. The murmur before noted is heard most distinctly when the patient is in the recumbent posture.

29*th*. Has improved to a certain extent; has lately had an attack of bronchial catarrh which has thrown him back. There is some lingering œdema of the lower extremities, though not nearly so marked as upon admission. Treatment unchanged. Patient was subsequently discharged very much relieved, the murmur having entirely disappeared.

CASE III. Annie G., æt. 19, American, domestic, admitted into Pennsylvania Hospital Oct. 12, 1871. Family of patient seems to be healthy. She began to live out four years ago. Her work has generally been light; her food has always been good; she has never had anxiety of any kind;

she began to menstruate at 15, and was always perfectly regular until one year and a half ago; she at that time missed one or two periods, and has ever since menstruated very irregularly. Soon after this functional derangement she became, as she expresses it, nervous; had headaches and backaches, and worried about herself. She gradually lost colour. The pallor of her skin progressed steadily, but her strength held out tolerably well until a few weeks before her admission to the hospital.

Condition on admission. Exceedingly anæmic; skin presents almost a waxy appearance; mucous membrane of lips and mouth white; appetite poor; bowels regular. Suffers from palpitation of the heart on exertion, a very faint hæmic murmur is heard at pulmonary cartilage; this is rendered considerably more distinct when patient is placed in the recumbent posture. Ordered the best diet that the hospital affords, and a teaspoonful of the following mixture: Liq. potass. arsenit. ℥ij; tr. nucis vomicæ ℥ss; vini ferri amari ℥viiss; to be taken three times daily.

Nov. 6. Her face has gained some little colour; is somewhat stronger, but improvement on the whole has been slow. A faint venous hum is heard over the jugular veins, but it is only heard when slight pressure is made with the stethoscope. The cardiac murmur is heard not merely at the pulmonary cartilage, but when the patient is lying down it is heard over the body of the heart and along the left edge of the sternum as far down as the ensiform cartilage.

In all these cases it will be noticed that the murmur became more intense upon the patient's assuming a recumbent position; in one case, indeed, it was occasionally only to be heard in this position, and in this case the venous hum was to be heard only when pressure was made with stethoscope over the course of the jugular vein, and the head of the patient was turned strongly to one side, so as to put the muscles of the other side on the stretch. I am, in consequence, inclined to believe, that, in many cases in which the anæmia is not marked, and in which no murmur can be heard when the patient is standing, it may sometimes be perceived by causing the patient to change his position. It will be recollected that in the passage quoted from Walshe's work on *Diseases of the Heart*, he calls attention to the fact that the anæmic murmur is modified in intensity by certain changes of position, but the context justifies the inference that these changes are of a nature to cause some excitement of the heart's action; whereas, in the cases reported by me, the change from the erect to the recumbent posture did not cause increased frequency of the pulse, which was, of course, noted carefully before and after having recourse to auscultation. The patients were also re-examined when they had risen from the bed, and in all these cases the murmur was then found to be less intense than it had been the moment before.

In a search through all the works on physical diagnosis which are at present accessible to me, I find but a single reference to this important point in the diagnosis of anæmia. And this is by an author whose book is indeed a standard one, but is also not as much read as it deserves to be. I allude to Dr. Stokes, of Dublin. When speaking of the murmur occasionally heard during the convalescence from maculated typhus, he mentions, as one of its characteristics, its frequent disappearance or diminution in the erect position, and says: "This may help us in distinguishing it from an organic murmur, but I am unable to say whether the same is not observable in ordinary cases of anæmia."

I am at a loss to explain the cause of the increased intensity of the murmur in the recumbent position. It is possible that the relation of the

heart to the large arteries at its base is of such a nature that increased friction of the blood against the sides of the arterial orifices takes place in this position, or it may be, that, the respiration being less free and more embarrassed in this than in the erect position, the circulation through the pulmonary vessels is interfered with, and, as a consequence, that a murmur may be produced at the pulmonary cartilage; or, admitting M. Parrot's theory to be correct, that this interference with the pulmonary circulation favours the occurrence of tricuspid regurgitation.

1872. *Jan.* 17. *Encysted Tumour of the Neck.*—Dr. WALTER F. ATLEE reported a case of this form of tumour occupying the submaxillary region on the left side.

The patient was a young man aged 25 years. The tumour was about the size of a goose-egg, and had existed for several years. For more than a year attempts had been made to effect a cure by means of a seton, injections of iodine, and by repeated tappings. Extirpation, which had been delayed on account of its extreme difficulties and dangers, was finally resorted to, and the patient recovered.

The chief interest in this case is derived from the fact that it appears to show that encysted tumours of the neck may have their origin in certain changes taking place in the lymphatic ganglia, as suggested by M. A. Richard (*Memoirs de la Societé de Cherugie*, t. iii.). Another reason for calling attention to the case is the fact that English surgical literature is most remarkably barren on the whole subject of tumours of the neck. In this respect Holmes's *System of Surgery* is quite deficient.

Feb. 7. *Report on Meteorology and Epidemics.*—Dr. WM. L. WELLS read the following report on meteorology and epidemics for 1871:—

From the meteorological record kept at the Pennsylvania Hospital, it appears that the most remarkable circumstance connected with the temperature of the past year was the extreme warmth of both March and April, a warmth unparalleled since the records have been begun in this city. The maximum temperature was on the 17th of March, when it reached 73°, and the minimum on the 29th when it fell to 34°. The maximum temperature of April was on the 9th when it reached 85°, and the minimum on the 2d when it fell to 38°.

The mean temperature of March was 48°.70 Fahr., or more than 7° above the average, and half a degree above March, 1859, the warmest previously known.

The mean temperature of April was 58°.15, Fahr., about 6½° above the average, and more than a degree and a half above April, 1844, the warmest previously known. Fortunately May also was warmer than usual, so that no injury was done to vegetation by frost.

The heat of the summer months, particularly that of August, was above the average, but it was far below that of last year, and consequently we find only 11 deaths from sunstroke, instead of 52 as in the year before, and a diminution of 173 in the deaths from cholera infantum.

The temperature of the last two months of the year was decidedly below the average: that of December being even lower than the average mean of January, and nearly 4° below the average mean of December for the last forty-seven years.

The statistics which follow have been furnished me by the kindness of Geo. E. Chambers, Esq., Registration Clerk of the Board of Health.

The total number of interments in Philadelphia in 1871 was 16,993,

a number exceeded only by the years 1864 and '65, when there were respectively 17,582 and 17,169 deaths, the great number then being caused chiefly by the deaths among soldiers, which amounted in the first year to 1598, and in the second to 778. The increase of 1871 over 1870 was 243, or 1.45 per cent. Deducting those who died elsewhere, and were counted because buried in Philadelphia (633 in number) we have 16,360 deaths, which makes (as the population of the city is 675,000), one death in 41.26, or 2.42 per cent.

Of the deaths recorded there were 8942 males and 8051 females, showing an excess of 891 males, or 11.06 per cent. The deaths of persons over 20 were 8251. Under 20 they amounted to 8742, or, deducting 875 stillborn, 7867. The preponderance of deaths among males was observed both in adults and in children, although a little greater in children, on account of the very great excess of stillborn males. Excluding the stillborn, the proportions of both sexes are about equal in the deaths of both adults and children.

The average of deaths for each quarter of the year is 4593; for each month 1431; for each day 46. The fourth quarter, which shows the highest mortality, had 44 per cent. deaths more than the second quarter, which had the least mortality. This is almost the reverse of the preceding year, when the fourth quarter, instead of having the greatest, had the least mortality, and the highest mortality was in the third quarter.

The mortality at the Blockley Hospital, connected with the Almshouse, was 401. That of the coloured population of the city, 957.

The epidemic diseases which in 1870 caused the greatest mortality either entirely disappeared, or very greatly diminished in the year just past. Thus scarlet fever showed a diminution of nearly 700, falling from 956 to 262. Relapsing fever nearly entirely disappeared, as only 7 died of it, instead of 162, which was the mortality of 1870. Of these, 3 were in January, being the last remaining of the epidemic of the preceding year. Yellow fever, which caused so much alarm in 1870, although the mortality was only 13, disappeared entirely in 1871, no cases originating in the city. These are only the most marked instances of what occurred also in all the other zymotic diseases, with one terrible exception. There were 100 fewer deaths from enteric fever, 52 less from croup, 27 less from diphtheria, 24 less from pertussis, 20 less from typhus, spotted fever, &c., 5 less from rubeola, 3 less from cholera, and 1 less from malarial fever, puerperal fever, and erysipelas, respectively.

The year in which these different diseases caused the maximum mortality was 1861 for scarlet fever and diphtheria, when 1190 died of the former, more than five times the mortality of last year, and 489 died of the latter, more than three times the mortality of last year; 1864 for typhus and malarial fevers, when 818 died of the former, nearly ten times the mortality of last year, and 145 died of the latter, or nearly three times that of last year; 1865 for enteric fever, when 773 died of it, two and a half times the mortality of last year; and 1866 for rubeola and cholera, when 221 died of the former, or more than five times the deaths of 1871, and 910 of the latter, instead of 4 deaths from cholera in 1871.

The diminished mortality from these diseases alone, as compared with the preceding year, was 1104. There was also a diminution of 123 in the deaths from diseases of the digestive system, and a diminution of 215 in the deaths from diseases of the nervous system; 34 less from old age, and 205 less from debility, over one year.

On the other hand, there was an increase of 67 in deaths from diseases of the circulatory system, and an increase of 35 in those from diseases of the respiratory system; there were 152 more deaths from congenital debility and malformation, and 53 more stillborn. The chief cause, however, of the total increase of 243 in the mortality was smallpox, which has been raging so fearfully in the past few months, causing in all 1879 deaths in 1871. This terrible disease has been very fatal, not only in this country, but in the British Islands, and on the continent of Europe, during the past year. In England the mortality in 1870 from smallpox was 1259, in 1871 it rose to 13,174, and in London alone 7876. In all England the proportion of deaths was 18 to 10,000 population, in London 24 to 10,000. (In Philadelphia 27 to 10,000.) In London the epidemic began in the end of 1870 and gradually increased until the beginning of May, after which it decreased, showing, however, a slight tendency to increase again in the last few weeks of the year. The mortality in the first quarter was 2400, in the second 3241, in the third 1255, and in the fourth 980. As it decreased in England it began on this side of the Atlantic. In Philadelphia it did not begin until the end of July. There were but 8 deaths from it in the first six months of the year, and in June, the last of these months, there were no deaths. In July there were 5 deaths, in August 16, in September 18, in October 331, in November 640, and in December 861, thus showing that even after the disease had fairly obtained a foothold, more than two months passed by before the mortality became suddenly alarming.

The manner in which the disease skipped about the city at first was remarkable. It began in the neighbourhood of 16th and Sansom, in August, passing in the same month to 17th and Pine. Of the 16 deaths in that month, 7 were in the 8th ward, where both these localities are situated. It is singular that in the following month, September, only 2 cases were reported in this ward, but a number more than a mile away, near 10th and Master, and 8th and Columbia Avenue, in the 20th ward, where not only no death but no case of the disease had occurred in the month preceding. In October it increased to an alarming extent in that ward, causing 89 deaths there, attacked the western part of Race Street, where also it was pretty severe, and established itself in all parts of the city, save only in Manayunk and its neighbourhood. In November its severity diminished one-fourth in the 20th ward, but doubled or nearly so everywhere else, and then for the first time it became really severe in the southern part of the city, where the most crowded streets and degraded and filthy population are found. In this month 6 cases were reported as originating in the 21st ward.

In December the disease was still more severe and still more uniformly diffused; diminishing considerably in violence only in the 20th ward, which it had attacked so overwhelmingly in October, and where it had already begun to diminish in November.

It is worthy of notice that the neighbourhood less attacked than any other and last attacked was the northwestern part of the city, Manayunk, and its neighbourhood, from which the prevailing winds blow in the cold weather, while the district earliest attacked, and most severely, was that part of the closely built up portions of the city nearest to this very healthy district.

In November and December the 3d and 4th wards were most severely attacked, while Germantown and West Philadelphia were, next to Manayunk, the most nearly exempt portions of the city.

The deaths among the coloured population were about in the same proportion as those among the whites.

As regards the age of those attacked, the young were especially liable to it. 836, or nearly half the whole number of deaths, were of persons under 10 years of age; 607, or nearly one-third, of persons under 5; 316, or about one-sixth, under 2 years; and 203, or nearly one-ninth of the whole number, were of infants under 1 year.

Between the ages of 10 and 20 only 323 died, being nearly 100 less than half the mortality in the preceding ten years. Between 20 and 30 there was an increase to 397, or nearly half the mortality in those under 10, and a little more than one-fifth of the total mortality. After 30 the risk of death rapidly decreased, the number of deaths in each succeeding period of ten years being, on the average, a little less than half that in the one before it, until we come to the period between 60 and 70, when there were 24 deaths, followed by only 7 in the next ten years, and 2 between 80 and 90, after which there were no deaths. This rapid diminution is of course far greater than can be accounted for by the smaller number of persons at a more advanced age.

A remarkable feature of this epidemic was the number of persons attacked who had had smallpox already, some even two or three times.

As regards the protection afforded by vaccination and revaccination, the statistics of the Board of Health are incomplete, but I have received the following particulars from W. M. Welch, M.D., physician in charge of the Municipal Hospital. In that institution there were treated in all in 1871, 1189 cases, of which 382, or nearly one-third, proved fatal,[1] 32 per cent. Of these, 390 had never been vaccinated, of whom 254 died, or 65.1 per cent. 331 had been vaccinated in infancy, and had a good mark, of whom 33 died, or 9.9 per cent.; 166 vaccinated in infancy—fair mark, of whom 27 died, or 16.2 per cent.; 302 vaccinated in infancy—poor mark, of whom 68 died, or 22 5 per cent. As regards revaccinated cases Dr. Welch furnishes no statistics, but says only that "smallpox after successful revaccination is in his experience exceedingly rare." As a large number of cases of variolous disease after revaccination have undoubtedly occurred in the city, this experience of Dr. Welch confirms the experience of others that such cases are generally very light, so much so as hardly ever to be taken to the hospital.

The only epidemic of smallpox which has equalled the present in severity since 1807 (when the records of the Board of Health begin), was in 1824, when there were 330 deaths, which, in proportion to the population of 135,000 or 140,000 at that time, gives about the same relative mortality as in the past year. No other epidemic has, in any one year, caused more than half the relative mortality of these two years.

The present epidemic was preceded by a period of remarkable exemption from the disease, during which, we may suppose, persons would become careless and neglectful of vaccination until smallpox, ever watchful of any such opportunity, would cruelly punish them for their thoughtlessness. A like period of exemption is to be found before the epidemic of 1824 (which began in the preceding year), as from 1820 to 1822, both inclusive, there was no death from the disease, and there was another period of exemption before the epidemic of 1861.

[1] Some of these deaths occurred in 1872, the cases having been admitted to the hospital in 1871.

REVIEWS.

ART. XV.—*The Principles and Practice of Surgery.* By JOHN ASHHURST, JR., M.D., Surgeon to the Episcopal Hospital, Surgeon to the Children's Hospital, etc. Illustrated with five hundred and thirty-three engravings on wood. 8vo., pp. 1011. Philadelphia: Henry C. Lea. 1871.

THE declared "object of this work is, as its title indicates, to furnish, in as concise a manner as may be compatible with clearness, a condensed but comprehensive description of the Modes of Practice now generally employed in the treatment of Surgical Affections, with a plain exposition of the principles upon which these modes of practice are based." There has been claimed for it, further, an American character, rendering "accessible to the profession at large the experience of practitioners of both hemispheres," "so much in this country having of late years been done for the advancement of surgical art and science, especially by the experience of the late war."

Any single volume of one thousand pages, embracing the Principles of Surgery and Practice as applied to the many general and local surgical diseases and injuries, must of necessity be, in the main, a work for students and general practitioners, and the chief questions to be answered in the estimating of its value, real and relative, are, how far does it present the pathological views of the day, and the practice most in accordance with those views as confirmed by the experience of surgeons competent to observe and decide. The answering of these questions necessitates an examination of the work somewhat in detail.

Of the forty-seven chapters into which the book is divided, two are devoted to "Inflammation;" two to "Operations in General and Minor Surgery;" two to "Amputations;" two to "Effects of Injuries in General, Wounds, and Gunshot Wounds;" eleven to "Surgical Injuries" of vessels, nerves, muscles, bones, joints, and special regions; and the remainder to "Surgical Diseases," local and general. The chapters on Inflammation certainly present a brief, plain, easily understood statement of the more modern doctrines. All sides of questions yet under discussion are given, and there is to be noticed a much-to-be-commended freedom from those positive dogmatic assertions of individual opinions as established laws and facts, which are to be found in some of the writings of the present time.

"Inflammation," our author says, "was formerly considered as a disease, an entity, a something superadded to the natural condition of the part. This view is now almost universally abandoned, and authors, though differing as to the proper explanation to be given of many of the phenomena of inflammation, are, I think, generally agreed that these phenomena are mere modifications of the phenomena of natural textural life. These changes, which are always due to the action of an irritant, no matter whence derived, may be observed as affect-

ing the phenomena respectively of function, nutrition, and formation, and in each the changes are primarily in the direction of excess."

As regards *function*, there is first increased activity, followed by perversion, and eventually, perhaps, by diminution or even total abolition. The *nutritive* changes are shown in an altered state of the vascular system of the part (as Mr. Simon has well put it: "A part does not inflame because it receives more blood. It receives more blood because it is inflamed"); in an altered state of the blood itself; in an altered condition of the parenchyma (temporary hypertrophy); and in a change as regards the neurotic condition, which doubtless reacts upon both vessels and parenchyma. The *formative* changes consist in the production of lymph and of pus. There may be also a destruction of existing tissue, resulting in its being thrown off as effete material, by the processes of ulceration and gangrene. Respecting the origin of the lymph-corpuscle and the fibrillous element of lymph,

"the two theories which at present chiefly divide the suffrages of pathologists are, (1) that which looks upon the new cellular elements as the result of proliferation of pre-existing cells, and (2) that which regards the cells of inflammatory lymph as identical with the white blood corpuscles and cells found in the lymphatic vessels, as identical, in fact, with the *wandering cells* which Recklinghausen has described as existing in connection with the ordinary connective-tissue corpuscles. * * * Hence, it would appear not improbable that both elements of inflammatory lymph may originate in pre-existing structures, the corpuscular from an increase in the number of wandering cells, from proliferation of the ordinary connective-tissue cells, or from both sources, and the fibrillous element from a transformation of the filamentary intercellular substance."

The kindred vexed question of the origin of the pus corpuscle is treated with similar fairness.

"In many cases (as in abscesses), the former [pus] seems to originate directly from the latter [lymph] by a simple liquefaction of the gelatinous intercellular substance of lymph; but in other instances the pus cell appears to have a different source. Virchow and other observers believe that pus-corpuscles originate from rapid proliferation (luxuriation) of connective-tissue and other nucleated cells; while Cohnheim, on the other hand, maintains that the sole origin of the pus-corpuscle is the migration by amœboid movement of the white blood-corpuscle through the vascular walls. Finally, Dr. Stricker and his able co-labourers, while acknowledging the origin of pus-cells from both these sources, have shown that the pus-corpuscles themselves divide and multiply, and that in profuse suppurations this is probably the chief mode of pus formation."

The remainder of the chapter is devoted to a "clinical view" of the causes, symptoms, course, and terminations of inflammation—well-known facts and phenomena being clearly yet briefly stated. Perhaps the most interesting point noticed is the reference to Dr. Meissner's experiments as appearing to "show that certain nerve fibres exercise a peculiar 'trophic' function, and that a lesion of such fibres may be the immediate and determining cause of an inflammatory condition of the parts supplied." In a recent lecture by Eulenburg, the same view is further maintained, nutritive changes, even to atrophy and hypertrophy, being attributed to trophic-nerve lesion.

"Experiment and pathology have demonstrated that alterations in the nutrition of the most different tissues—epidermoid, subcutaneous, muscular, osseous, articular, lymphatic—ensue in consequence of artificial or accidental injuries of nerves. Without mentioning the numerous experiments performed in proof, I will only call to mind the phenomena following gunshot injuries or other trau-

mata causing partial or total destruction of mixed nerve trunks. Our late war has furnished abundant material for this purpose, and I have myself observed the most exquisite examples in proof. * * As Fischer has lately exhibited, œdema and erysipelatous reddening (the shiny fingers and toes of Paget), further, anomalies in the growth of the hair, exanthemata, anchylotic joint inflammations, contractions, hyperplasia, and later, concentric atrophy of the bones have all been observed after injury of the nerves of the extremities. To refer all these alterations exclusively to injury of the vaso-motor nerve fibres would be a labor of the greatest difficulty; for we know that elevation of temperature, reddening, and increase of secretion occur after division of the vaso-motors—effects which, after injuries of mixed nerve trunks, either do not present at all or only transitorily, and, as a rule, soon to manifest opposite appearances. There must be, therefore, some further condition which compensates or more than compensates for the injury of the vaso-motors, and this, in the injury of mixed nerve trunks, is the *concomitant injury of the secretory, and in a narrower sense, the trophic nerve fibres.* * * *

"The one-sided progressive atrophy of the face occurring occasionally as an isolated disease it is certainly unreasonable to refer to vaso-motor cramp of a year's duration; as little, too, are evidences of a relaxation of vessels present, so that we must necessarily recur here to disturbances in the trophic system, as Samuel has long ago justly maintained. It is worthy of mention, too, that hypertrophy of one side of the face may occur in connection with injuries of nerves; Sterling has already communicated an interesting case of this kind. This coincides perfectly with the experiments of Mantegazza, who has observed hypertrophy of the connective-tissue, of the periosteum, the bones, lymph glands, etc., in animals after section of the nerves."

Chapter II: "Treatment of Inflammation," hygienic, local and general. Some very judicious remarks are made upon the *diet* of patients suffering from inflammation, and Dr. Ashhurst is evidently no believer in the starvation plan.

"It has been, I think, clearly shown by the researches of modern investigators, that in addition to the waste of tissue, which accompanies the inflammatory process, there is a large expenditure of force (as evidenced by the great elevation of temperature), and it is but rational to suppose that this waste and expenditure ought to be compensated for by a supply of easily-assimilable food. As to the results met with in practice, it of course becomes any one surgeon to speak with great modesty and hesitation; I can, however, honestly aver that I have met with better success in the treatment of inflammation upon this plan, than I did when I habitually directed low diet, according to the rules still laid down in many surgical works."

In the local treatment, special attention is called to "mediate irrigation" as described by Petitgand. Practically we are not familiar with it, but theoretically the plan commends itself, as safe, certain, and easily regulated; the which cannot be said for the ordinary methods of applying cold and heat. Poultices are more highly esteemed than they have been by many writers since the time of Liston. When the patient or nurse will see to it that they are properly made and applied, they are unquestionably of great service; but when, as is too often the case, they are badly made and irregularly changed, both surgeon and patient will be much better satisfied with the use of warm water and oiled silk. In the "Constitutional treatment," the debatable points are bloodletting and the use of mercurials. Our author has decided opinions, in harmony, too, with the great majority of the better surgeons of the day.

"It seems to me but reasonable that we should adopt the same principles in the management of traumatic inflammations that we do in dealing with those of idiopathic origin; and hence, that venesection should not be resorted to in

the treatment of surgical affections, except for its immediate mechanical effect in relieving a vital part, the functional or structural integrity of which is in imminent danger. For example, bleeding may be necessary in a case of traumatic as in a case of ordinary apoplexy [under circumstances stated]. * * * Or when a wound of the lung is followed by great dyspnœa, pain and oppression, the loss of a little blood may be of benefit, just as it would at the outset of an ordinary pneumonia presenting similar conditions. Even under these circumstances, I believe local bleeding, by cupping or leeching, will be usually better than venesection; and it should always be considered that the loss of blood is an evil, which may indeed be preferable to a greater evil, but is never a positive good."

The mercurials he holds in higher esteem than do many of his fellow-workers and writers.

"I cannot but believe," he says, "that it does exercise an influence, particularly over the second stage of inflammation, or that attended with the production of lymph. It is, however, like bloodletting and antimony, a dangerous remedy, and a positive evil, though it may on occasion do good. It should, I think, be reserved for cases in which an important organ is endangered, and should even then be used with great caution and reserve. It is especially adapted for inflammations of fibrous and serous membranes, such as the meninges and peritoneum. It should be given in small doses, as $\frac{1}{4}$—$\frac{1}{2}$ gr. of calomel, or $\frac{1}{2}$ gr. of blue mass, and may be conveniently combined with opium and ipecacuanha."

The latest English writer on surgery (Gant) thus presents what is, perhaps, the most prevalent view:—

"Formerly, also, and until recently, it was unanimously held, that the same influence prevented or retarded the effusion of fibrin, and promoted its absorption. * * * Long as this therapeutic theory prevailed, and practice accordingly, there are reasons for doubting the accuracy of the one and the efficacy of the other. That mercury exercises some kind of an influence over nutrition there can be no doubt; but it is questionable how far the natural course of an inflammation, having induced the effusion of fibrin, may itself tend to absorption, apart from the co-operation of mercury."

The tendency seems to be to the restricting of the use of mercurials to the accomplishment of two purposes, the production of catharsis and the peculiar effect of the agent upon syphilitic manifestations.

Chapter III.: "Operations in General; Anæsthetics." The general remarks, while pertinent, need not especially engage present attention. Those on anæsthetics deserve the careful consideration of all readers of the book. The danger of anæsthetics being recognized, cases in which they should be employed are pointed out and indiscriminate use forcibly warned against.

"It is to be feared that students and young practitioners often get a false impression upon this point, and from seeing the frequency and apparent profusion with which anæsthetics are administered by their clinical teachers, derive a notion that these agents are perfectly harmless, and may be indiscriminately resorted to under all circumstances. The true rule upon this matter (a rule which is, indeed, applicable to all our perturbing modes of treatment) is, that when anæsthetics are not positively beneficial, they are injurious. Hence, under ordinary circumstances, they should not, I think, be employed, except for really important operations, and those which without their use would be tedious and painful. It is seldom right to give anæsthetics for purposes of diagnosis merely; there are, however, parts of the body the lesions of which are so obscure, and in dealing with which a mistaken diagnosis might lead to such grave errors of treatment, that it is often not only justifiable, but even imperative to employ anæsthesia in their examination. * * * Cases for what are

called *capital operations* (where life is immediately involved), are almost invariably cases for anæsthesia; for smaller operations, the practice should vary according to the time required for their performance; thus, anæsthetics should be given before operating for piles, or for phimosis, for these are tedious procedures; while opening an abscess, cutting an anal fistula, or tapping a hydrocele, is quickly done, and does not require the use of these agents."

The comparative merits of ether and chloroform are thus stated:—

"Either agent has some advantages over the other, and some corresponding disadvantages. Chloroform is more prompt in its effects than ether, the patient is usually quieter while coming under its influence, it is less apt to cause vomiting, a smaller quantity than of ether is required to produce anæsthesia, and the patient reacts more quickly when the inhalation is stopped. It, however, requires much greater care in its administration than ether, and its use is attended with much greater risk to life. The above statement gives my own estimate of the relative merits of these agents, and, I believe, corresponds pretty closely with the opinions usually entertained on the subject." * * *

Mr. Spencer Wells, at a late meeting of the Medico-Chir. Society, objecting to chloroform, especially on account of its vomiting, to ether because it was so troublesome, declared his preference for bichloride of methylene, regarding it as the best anæsthetic.

Dr. Ashhurst's preference for ether is, we believe, not shared by all surgeons, and it must be confessed that the rapidly increasing list of deaths from the administration of chloroform is causing many an operator to question the propriety of continuing the use of his hitherto favourite anæsthetic agent. In this connection we are surprised to see no reference made to the preliminary subcutaneous injection of morphia when chloroform is to be given. It certainly seems in many cases to render necessary a much smaller quantity of chloroform, and to prevent to a large extent the occurrence of vomiting.

Chapter V. treats of "Amputations in General," the preparation of the patient therefor, the method of operating, the diseases of the stump, the comparative mortality of different amputations, and the causes of death.

Respecting the relative merits of the various methods of amputating, it is written—

"If any general rule were to be given, I should say that the circular incision or Teale's method gives the best stump in the forearm, the modified circular in the upper-arm and the upper part of the thigh, the common double-flap operation immediately above and below the knee, the circular or lateral flap in the lower part of the leg, and the oval operation at the joints."

This estimate seems to be a very just one, and there certainly exists, particularly among many civil surgeons, an undue prejudice against the circular and modified circular operations. The paragraph on "Mortality after Amputations" contains a reference to Dr. Addinell Hewson's investigations upon barometric variations as affecting the death-rate, and statistical tables of the relative mortality after amputations according to locality, cause, and time of operation, tables compiled with much labour and care, and that will be valuable to surgeons for future reference.

It is a little surprising to see that the percentage of deaths after amputations of the leg in civil hospitals is higher than in military surgery in the proportion of about 4 to 3. With reference to early as compared with late amputations our author says:—

"It is now, I believe, universally acknowledged among military surgeons that primary amputations (except of the hip-joint and the upper part of the

thigh) do better than others; of those which are not primary, the secondary do better than the intermediate. It is, however, commonly said that in *civil* practice secondary amputations are more successful than primary, and this difference has been accounted for by the different hygienic circumstances by which soldiers and civilians are respectively surrounded. I believe that the usual statement upon this point is erroneous, and that a careful collation of statistics will show that in both civil and military practice, primary amputations are followed by better results than others."

In an abstract of a report on the military service of the late Franco-German war, by Pirogoff, we find the following, bearing on this question:—

"With this opportunity, he takes occasion to differ with Langenbeck, who stated, at the 74th anniversary of the Friedrich Wilhelm Institute, that field service could only be of use to the severely wounded when it began immediately after the battle and ceased not later than two days after it; all unavoidable amputations, therefore, must be performed in the first 12–24 hours after the battle. The later and intermediate amputations give the most unfavorable mortality per cent.

"Against this view, Pirogoff maintains that conservative treatment and late amputation are so indissolubly united that if the possibility of the first be granted, the probability of benefit from the last must be acknowledged. The former doctrine of Bouchet and Larrey was consistent; it demanded that amputations should be as quickly performed as possible, because it claimed that the preservation of limbs was in general more dangerous than their amputation, and taught that whoever did not amputate immediately after an injury ran the risk, in the attempt of saving more limbs, of losing more lives. So soon, however, as conservative surgery is considered admissible into military service, so soon, to be consistent, must protest be entered against early amputations. To defend conservative surgery and at the same time to advocate early amputation is to be inconsistent at once. In considering the favourable course of early in comparison with the unfavourable course of later amputations, it must not be forgotten that it was only the upper extremity that furnished this favourable result. And here conservative treatment has been attended by the best results. In the lower extremity, early as well as late amputations show the same unfavourable results. Whoever here gives preference to late over early resection must give the same preference to late over early amputations. In a word, the introduction of conservative treatment in modern military surgery is of itself a protest against early amputation, which of course admits of no preservation of the limbs."

The paragraph on the causes of death after amputations closes in these words:—

"*Pyæmia* is the chief cause of death after pathological amputations, after those of expediency, and after primary amputations for injury.

"*Exhaustion* is the chief cause of death after secondary amputations for injury, and ranks next to pyæmia as a cause of death after the primary, and those classed as pathological.

"Mr. Birkett, from a study of 171 cases, in which the operation was performed either by himself or under his direction, concludes that 'a large proportion of the patients submitted to amputation, when inmates of a metropolitan hospital, are the subjects of more or less advanced chronic disease of the thoracic or abdominal viscera,' and that 'the chances of death after operations appear to depend almost entirely upon the previous state of each patient's constitution.'"

Chapter VI. treats of "Special Amputations," the chief operations in the various localities being duly described. Hip-joint amputations, with tabulated results, occupy four pages; probably more space than they are entitled to, considering the rarity of their performance. We regret, however, that, as so much was written, a few lines more had not

been added, making reference to the method of Lacauchie; a method by which the disadvantages of the abdominal tourniquet can be avoided, hemorrhage being controlled by compression of the femoral artery.

Chapter VII.: "Effects of Injuries in General, and Wounds;" embracing the consideration of shock, its nature and treatment, traumatic delirium, the local effects of injuries, the classification and treatment of wounds. Considerable attention is paid to the so-called "antiseptic treatment," and the value of it is summed up as follows:—

"As has already been said, the merits of the antiseptic method cannot yet be considered as positively determined. Failure on the part of other surgeons to attain the expected results, is attributed by the advocates of the plan to some mistake or neglect in the application; and such may doubtless be the case. But it is obvious that any mode of treatment which is so intricate and complicated as to elude the skill of such excellent surgeons as have failed with the antiseptic dressing, is not likely ever to be adapted for general employment."

Is it this intricacy and complicated nature of the antiseptic dressing that causes failure? Is it not, in the great majority of cases, want of care in applying a not very complicated dressing, and unwillingness to devote to such application the requisite time. Antiseptic dressing does not necessarily mean carbolic acid dressing, but that which will prevent the access to the surface of a wound or ulcer of undisinfected air.

Prof. Lister chiefly employs, it is true, carbolized dressings, but prefers the chloride of zinc solution as a primary application to wounds already open. A year ago he spoke favourably of the use of cotton, either simple or carbolized, on account of its well-known power of filtering air of contained organic germs; and Guérin has claimed that raw cotton is the best of all dressings after amputations, best because of its easy application, and complete prevention of pus-decompositions. Observation of civil and military hospitals has never elsewhere shown us such healthy appearing wounds, and such freedom from decomposed, irritating secretions, as in Lister's own wards; and certainly the extraordinarily successful issue of his cases cannot be attributed to the special favourableness of the hygienic surroundings of those under treatment in the Edinburgh Infirmary.

Chapter VIII. is devoted to "Gunshot Wounds," a subject unfortunately very familiar to American surgeons. There are stated, as usual, the once characteristic appearances of the entrance and exit wounds. It was noticeable in our war, and still more in the continental wars since, that with the improvement in precision of the arms employed, and the changes made in the shape and weight of the bullet, the differences in these wound-appearances have become much less marked; so much so indeed that Pirogoff has declared that, in the Franco-German war, "the orifices of entrance and exit could not be distinguished, both openings presented the same appearance." In reference to excision in the upper extremity, Dr. Ashhurst says:—

"It may now be said that in most cases of injury of this part of the body, excision should be the surgeon's first thought, and should be preferred to amputation whenever the destruction of parts does not manifestly render the latter operation imperative. * * *

"Primary excision is the best mode of treatment for gunshot injuries of the shoulder-joint. * * *

"Excision of the elbow will still be an operation to be recommended in military practice, for the prospect of preserving a useful arm fully justifies the surgeon in incurring the slight additional risk over that of an amputation.

"Excision of the wrist-joint has not been much practised in military surgery; the results of such operations as are recorded have been sufficiently satisfactory as regards life, but rather unsatisfactory as regards the utility of the preserved limb."

These are certainly the views of the great majority of surgeons, and the experience of our war justifies them. We find, however, that Pirogoff, in the war of 1870, found "some fifty cases of recovery of gunshot wounds of the shoulder, elbow, and ankle without resection, a fact which is significant against the adoption of early resections." Respecting wounds of the knee-joint our author agrees with Guthrie in the opinion that they require amputation; the choice being regarded as lying between this operation and excision of the knee. This latter operation, with its mortality of over 75 per cent. (fatal in every case that we have personally observed), "should be banished from the practice of military surgery." But should the "rule be regarded as imperative that every gunshot fracture of the knee-joint is a case for amputation?" We have thought so, though we have seen a very few cases of recovery without either amputation or excision. But we now find it stated in the abstract already several times referred to, that

"What neither he [Pirogoff] nor any other surgeon [?] could have anticipated was the so extraordinarily happy termination of a great number of cases of gunshot wound of the knee-joint.[1] He counted forty such fortunate cases; in some cases, indeed, even motion was maintained in the penetrated joint, and in two instances of recovery the ball was still in the bone. He ascribes these favourable results to the smallness of the chassepot projectile and the direction of the wound, from before backwards."

As was to have been expected, in the preparation of this chapter free use has been made of the very valuable circulars issued from the Surgeon-General's Office.

Chapter IX.: "Injuries of Bloodvessels." For the arrest of hemorrhage the ligature is preferred to either acupressure or torsion, and the surgical world at large is of the same opinion. It requires no little courage and faith on the part of an operator to lay aside the ligature, and trust to the needle, or torsion, for the proper closure of a large artery. Whether or not the recognized disadvantages of the ligature can be wholly overcome by the employment of antiseptic animal ligatures, and whether the latter will ever come into general use, remains to be seen. About seven pages are devoted to the "lines of incision for deligation of special arteries," more than half of which space is taken up with woodcuts. We may express our doubts of the value of such cuts in a work on surgery proper, or indeed in any work; for unless one knows well the anatomy of the several regions he will not be able to derive any benefit from them, and if he does know such anatomy he will not need the illustrations.

Chapters XI., XII.: "Fractures." The various definitions, explana-

[1] "Neudorfer in his writings cites repeatedly cases of recovery after gunshot wounds of the knee-joint, some of them with partial use of the injured joint. It is stated also in the appendix (p. 224) "we may now cherish the hope as well ounded, if it should become an axiom in military surgery in cases of injury to the knee-joint to apply an immediate plaster bandage instead of resorting as hitherto to immediate amputation, that the number of cases of recovery would be much greater; might be cited, indeed, as of daily occurrence, instead of being regarded as rarities and fortunate exceptions to a rule."

tions, and directions are clearly given in as few words as the subject will permit. Ununited fractures are considered somewhat in detail, upon which subject the literature of the profession has lately been increased by the very valuable monograph of Berenger-Feraud. In the treatment of fractures of the clavicle, we find the use of an axillary pad condemned; as we think wisely. How it has managed to hold, as long as it has, a place in the surgeon's armamentarium we have never been able to understand, for when by its position of any special service its pressure is so great as to be intolerable. The simple adhesive-plaster dressing is not only the simplest but the best, a dressing also applicable to fractures of the several parts of the scapula.

The pistol-shaped splint receives the usual commendation in the treatment of fractures of the lower extremity of the radius; why, we fail to understand; for the possible securing by its employment of any theoretical benefit we have found to be accompanied by the causing to the patient of no little practical discomfort and pain. By some oversight no mention is made of Prof. Moore's (of Buffalo) interesting and valuable experiments and practical conclusions deduced therefrom; an omission which we hope to see corrected in a second edition. Preference is expressed for the "sand-bag and extension" treatment, the "American method," in fractures of the femur. The cumbrous long splints with their wretched perineal bands ought to be consigned to "Museums of Antiquities."

Chapter XIII.: "Dislocations." To the ordinary dislocations of the head of the radius should be added that form of displacement to which attention has lately been called by Dr. Lyell; occurring in young children, especially girls, characterized in the two cases that we have seen by semi-pronation of the hand, pain only when an attempt is made to pronate and supinate, unimpaired motions of elbow and wrist; the difficulty being entirely, and at once, relieved by strongly supinating the hand, and making gentle pressure over the head of the radius. In treating of hip dislocations, due regard has been had to the experiments of Bigelow as set forth in his masterly monograph.

Chapter XV.: "Injuries of the Head:" a chapter written with much care, on a subject that has evidently engaged no little of Dr. Ashhurst's thought and attention. In reference to "Concussion," he says:—

"Every case of concussion is, I believe, accompanied with *shock*, and in many instances the symptoms of the latter condition alone can be recognized. * * * Even in the intermediate cases, which are often spoken of as typical instances of concussion, though, as a matter of convenience, we may trace their clinical history, and divide it into stages, we cannot point to any symptoms which definitely characterize the lesions of concussion, apart from those of other cerebral injuries. Indeed it would be better, I think, if we could dispense altogether with the term concussion as denoting a *condition*, and look upon it as merely indicating the *cause* of what have been described as *concussion lesions*, viz.: *cerebral contusion, laceration, extravasation.* * * * I regret that I cannot agree with those surgeons who consider the diagnostic marks between compression and concussion to be plain and easily recognizable. * * * Though in certain cases we can say without hesitation, in view of the one-sided paralysis, profound coma, and other symptoms mentioned, this is compression or that is concussion, there are other cases in which it is impossible to draw such a distinction; compression may disappear spontaneously, leaving concussion, while concussion, by a continuance of intra-cranial hemorrhage, may end in fatal compression."

Of its treatment he says:—

"As a matter of fact it is very seldom indeed that a case of concussion requires any stimulus at all. * * * [After reaction has taken place], rest, both mechanical and physiological, should still be enforced; and if the patient be restless, the surgeon need not fear to give opium. I am aware that there is a good deal of difference of opinion as to the propriety of administering opium in injuries of the head, but surely there is nothing to contraindicate it in what we know of the pathology of these cases, while its soothing and calming effect is exactly what is required."

He "would restrict the use of the trephine within very narrow limits; it is not to be used with the idea of relieving compression, nor with the idea that there is any special virtue in the operation, to prevent encephalitis. The trephine should be used merely as Hey's saw is used, mechanically, to enlarge an opening which would be otherwise too small to allow the surgeon to carry out plain therapeutic indications."

Upon its use in extravasations, in intracranial suppurations, and in cases of epilepsy, his views are eminently judicious and in harmony with the teachings of the day.

Chapter XVI.: "Injuries of the Back." A table is here given of 36 resections of the spine for fracture, of which 29 resulted fatally, 3 were relieved, in 2 no benefit was received, and in 4 the result is unknown. "The operation, far from increasing, positively diminishes the chances of recovery."

Chapter XVII.: "Injuries of the Face and Neck." Of "foreign bodies in the air-passages" the mortality is stated to be "in general terms, as nearly as may be, 1 in 3, the death-rate *after operation* being less than 1 in 4 (24.8 per cent.), but *without operation*, more than 2 in 5 (42.5 per cent.). The period during which a foreign body may remain in the air-passages, and yet be spontaneously expelled, varies from a few hours up to many years. In 64 of 124 cases of spontaneous expulsion with recovery, collected by Mr. Durham, this period was between one and twelve months."

Respecting the relative advantages of laryngotomy and tracheotomy, our author says:—

"While I do not believe that any rule of universal application can be safely laid down upon this question, I would advise in general terms, that *tracheotomy above the isthmus* should be preferred in all cases in which time is afforded for a careful and deliberate operation; but that if great haste be essential, *laryngotomy* * * * should be performed instead. When the operation is required by the presence of a foreign body in the windpipe, a more definite rule may be given. If the offending substance be lodged in the larynx, that part itself must be opened; but if the foreign body be in any other part of the air-passages, tracheotomy is the operation to be chosen."

Chapter XVIII.: "Injuries of the Chest." In the paragraph on the treatment of lung wounds, the "hermetical sealing, or Howard's method," is condemned as usual; as is also venesection, the use of which proved in our late war to be unnecessary and injurious, and according to Neudorfer the same was found to be true in the Franco-German war. Many an American surgeon, however, remembers how he once went forth, believing that in cases of wound of the lung, complicated with severe internal hemorrhage, he was to tie up the arms, and bleed "*pleno rivo ad deliquium.*"

Chapter XIX.: "Injuries of the Abdomen and Pelvis." Contusions of the abdomen, attended with laceration of the abdominal viscera, are pronounced, "in the majority of instances, necessarily fatal." In this connection an intimation might have been given, for the benefit of the student and younger practitioner, that the visceral lacerations may occur without

there being any external indication of injury. In one case under our own observation, a man having been run over by a horse-car, there were no external appearances of abdominal injury, yet he died in 2½ hours, and post-mortem examination revealed lacerations of the stomach, duodenum, liver, spleen, and both kidneys. The diagnosis of penetrating from non-penetrating wounds is declared "often difficult, and any exploration with a probe would be manifestly improper." Yet we hear, not very unfrequently, of surgeons of eminence probing (once even to the extent of *twenty-two inches*), gunshot wounds of the abdomen, in which there has been penetration and wounding of the contained viscera. Might it not be well for these surgeons to content themselves with a little less accuracy of diagnosis, that they might afford their patients a little more chance of recovery? In cases of the female catheter slipping from the fingers and being sucked into the bladder, we find it judiciously advised to "*at once* dilate the urethra and remove the foreign body. I have known fatal ulceration to result, under these circumstances, from the delay of only a few days."

Chapter XX.: "Diseases Resulting from Inflammation." Though the classification of ulcers is simpler than that in many similar works, we should have preferred seeing the yet simpler division into acute and chronic, the classification adopted by Prof. Gross. As to "transplantation of cuticle," Dr. Ashhurst is not so much of an enthusiast as some of his fellow surgeons at home and abroad.

Considerable space is occupied in the consideration of "hospital gangrene," but not more than we should expect in an American work published so soon after the close of the war. Our own experience was fortunately very limited, but, in the few cases under our care, the *permanganate of potassa* answered every indication; and we can agree with Dr. Ashhurst in his statement that "I have never as yet been disappointed in its effect; it is but just, however, to say that I have not had occasion to try it in any cases of the worst form, such as are described by Guthrie."

In the treatment of "carbuncle," the use of the knife is not advised, adhesive plasters and pressure being recommended. A few years ago, it will be remembered, considerable attention was attracted to the external application of castor oil and turpentine combined, a method of treatment which certainly succeeds well in many cases.

Chapter XXI.: "Erysipelas." The differential diagnosis is clearly and succinctly given; the treatment recommended, judicious. It might be suggested that the tr. ferri chloridi may be advantageously pushed beyond the limit stated (♏xx to ♏xxx); the dose can be safely increased to ʒj -ʒij. In severe cases of the phlegmonous variety, multiple incisions are recommended. Will it not be safer in many cases to make a single long free incision, and avoid the risk of the skin between the many small incisions becoming gangrenous, as it will sometimes?

Chapter XXII.: "Pyæmia." As to the pathology of this "bane of operative surgery," our author considers the most plausible theory, "that which makes it dependent on the introduction of a septic material into the blood, and which looks upon the processes of thrombosis and embolism as subsidiary and not absolutely necessary. * * * * We are thus brought to the conclusion, that the only theory which is capable of accounting for all the phenomena of pyæmia, is that which supposes the pyæmic condition to be induced by the absorption of the septic material (usually in a liquid, but possibly sometimes in a gaseous state), which

unfits the blood for the processes of healthy nutrition, induces capillary stagnation and its consequences, low forms of inflammation, or serous and synovial effusions, and may, and probably does in most cases, cause venous thrombosis, giving rise to the occurrence of loose and ill-formed coagula, which, rapidly undergoing disintegration, cause capillary embolism, and thus produce the secondary deposits, or metastatic abscesses, which are so common in this affection."

In the late discussion in the French Academy, Verneuil declared that "purulent infection ends in suppuration but does not arise from it," and that there can be "no pyæmia without sepsine and emboli," (the limited application by the French of the terms pyæmia and purulent infection will be remembered).

Chapter XXIII.: "Diathetic Diseases" (Struma, Rickets). Passing over the general remarks on struma and the pathology of tubercle and scrofula; the relations of the latter diseases to operations are thus stated:—

For "operative interference in tuberculous cases, no general rule can be given. The prognosis of an amputation or excision for tuberculous disease, is undoubtedly less favourable than that of a similar operation for scrofulous, or simple chronic inflammation. If there be evidence of tuberculosis of internal organs, any operation should, as a rule, be avoided; the only exceptions are—(1) when it appears that the visceral disease is caused by the external affection, and when, therefore, there would be reason to hope that by removing the latter the progress of the former might be checked; and (2) when the patient's suffering from the external disease is so great, that the operation is called for simply for the relief of pain. * * * With regard to operations in *scrofulous* cases, no rule of universal application can be laid down. I am decidedly of the opinion that, in the immense majority of instances, enlarged cervical glands should not be interfered with. * * * With regard to operations for scrofulous bone and joint disease, the question is more doubtful. As a rule it may be stated that no operation should be performed, while a reasonable hope remains that a cure can be effected by expectant treatment; if, however, the powers of nature should be manifestly incompetent for the task, or if (as is often the case among patients of the poorer classes) the time which would probably be required for a natural cure be an important consideration, operative measures may be properly resorted to, and will often be followed by the most gratifying results. Excision is of course preferable to amputation, when the circumstances of the case permit a choice."

Two pages are devoted to rickets, a disease which is, very possibly, more common among us in its milder forms than is generally believed, and upon which valuable papers have of late been given to the profession by Jacobi, of New York, and Parry, of Philadelphia.

Chapters XXIV., XXV.: "Venereal Diseases." These, Dr. Ashhurst, in common with most of the more recent writers, regards as *three* in number: *gonorrhœa*, *chancroid*, and *syphilis*. The recommended treatment of the first is wholly or chiefly by injection, a method which is doubly valuable when it is practicable for the surgeon to secure a thorough preliminary cleansing of the urethra and the contact of the medicated solution with all the affected part; almost impossible when the ordinary syringe is employed, but perfectly and easily accomplished by the use of a "reflux catheter."

The most interesting question connected with the two remaining affections is as to their relationship. The "dual theory" is very generally accepted by writers, though the diversity of chancroid and syphilis is, "even now, denied by a good many surgeons, and is practically ignored by a still larger number." But there are some phenomena, in some cases,

which it is very difficult to reconcile with the declared entire distinctness of the two diseases Explanations are given, and attempts made to square facts with theory, but it seems as though sometimes more credulity was required for the acceptance of the explanations of the "dualists," than the theory of the "unicists." It is possible that an early and exact diagnosis of syphilis may yet prove obtainable by the microscopic examination of the blood. It will not be forgotten that Dr. Salisbury declared, four years ago, that he had discovered algoid forms pathognomonic of syphilis; and now we have Lostorfer announcing the discovery of corpuscles in the blood of syphilitic patients, so characteristic of the disease, and so confined to the subjects of it, that he is able simply by microscopic blood-examination, to determine who is and who is not syphilitic; and Lostorfer's honesty and accuracy are vouched for by Skoda, Stricker, and Hebra.

Chapter XXVI.: "Tumours." The classification adopted is the usual one of English writers of the present day, viz.: non-malignant and malignant; the former including cystic tumours, solid tumours and outgrowths; the latter, cancer and epithelioma. Respecting the local origin of cancer, Dr. Ashhurst expresses himself as follows:—

"That a local cause, traumatic or otherwise, can, without any previous predisposition on the patient's part, give rise to the formation of a cancer, it is hard to believe; at the same time, cancer may undoubtedly (from a practical point of view) be looked upon as, at first, a local affection, its early manifestations being of a local nature, and the only applicable treatment being of a topical character; even when 'cachexia' precedes the appearance of a cancerous tumour, the removal of the latter may relieve, at least temporarily, the cachectic condition."

In this connection and bearing upon the question of the relationship of epithelioma and other forms of carcinoma, it is interesting to notice Billroth's declaration, that "further observations, with new aids, have made it appear to me more probable that, even in the smallest cancerous nodules, epithelial elements always gave the first start for development. This has been confirmed by Waldeyer."

Chapter XXIX.: "Surgical Diseases of the Vascular System." The paragraphs on aneurism, constituting the greater part of the chapter, are interesting, and the statistical tables given will be found of much service to later workers in the same field. With reference to the comparative merits of ligation and compression, it is held that certainty so far as the cure of the aneurism is concerned, and danger to life, are on the one side, safety and greater liability to failure on the other. Compression is to be preferred—

"(1) In all cases in which, from the age or general condition of the patient—from the existence of heart disease, or other aneurisms, or of marked structural change of the arterial coats—or from the prevalence of erysipelas, pyæmia, etc., the operation of ligation would be attended by particular risk; (2) in all cases in which the aneurism, being detected at an early stage, would be in the most favourable condition for the use of compression, and in which the pressure treatment, if even it failed, would not seriously lessen the prospect of benefit from subsequent ligation; and (3) in all cases, on the other hand, in which the aneurism, from its locality or size, would not probably be amenable to the ligature, and in which, therefore, pressure should be at least tried, before resorting to such formidable measures as amputation, or the 'old operation.'"

Chapter XXX.: "Diseases of Bones."

Chapter XXXI.: "Diseases of Joints." Here again we have carefully written condensed statements of what is at present known of the

pathology of the diseases in question, and of the various methods of treatment in themselves and compared with each other.

Chapter XXXII.: "Excisions." To those for whose use the work under review is especially to be recommended, viz., students and general practitioners, the entire chapter will well repay careful study and remembrance; to surgeons it is not less valuable on account of its statistics.

Chapter XXXIII.: "Orthopædic Surgery." For the relief of the various forms of club-foot (diseases held to arise from the contraction of certain muscles or the paralysis of their antagonists), Dr. Ashhurst speaks more highly of tenotomy than do some who have written of late years on the subject. Of course if spastic contraction of a tendon or a set of tendons is the essential element of the disease in a given case, then section of such tendon or tendons will be the first and most important step towards remedying the deformity, but if paralysis from intra-uterine disease be the cause, tenotomy will in a large proportion of cases be unnecessary and improper. We should have expected, considering the proposed "American character" of this work, that its author would have noticed, at least by name, some of the orthopædic surgeons of our country.

Chapters XXXV., XXXVI.: "Diseases of the Eye and Ear." As might have been expected from the size and scope of the work, the various subjects touched upon in these chapters are hardly more than simply touched upon, thus giving the chapters much of a dictionary-like character. What, however, has been written is, so far as it goes, in harmony with the teachings of the best ophthalmic and aural surgeons of the time. The special fields of ophthalmology, otology, and gynæcology have been so widely extended during the past twenty years that in no single volume on general surgery can anything other than a bird's-eye view of them be taken.

Chapters XL., XLI.: "Hernia." The various operations for the "radical cure" having been referred to, their value is thus summed up:—

> "Hence, it would appear that the most favourable view to be taken of any operation which has been as yet proposed for the radical cure of hernia, is that while not necessarily followed by grave consequences, it is not unattended with risk, and by no means certain to effect the object designed; and that in consideration of the safe and really satisfactory means of palliation afforded by the use of a truss, the radical treatment must be looked upon as at best an operation of complaisance or expediency. * * * Occasionally justifiable, the operation is not, in any case, one to be urged upon the patient, nor to be performed without his fully understanding the uncertainties and dangers of the procedure; and in the immense majority of cases the surgeon will best consult both his own reputation and the good of his patient, by dissuading from any but palliative treatment."

As an important sign in establishing the diagnosis of strangulation, as distinguished from incarceration, peritonitis, and vomiting from other than hernial causes, attention is called to the absence of impulse on coughing, the limit of impulse being further of service as a guide to the location of the seat of strangulation. The use of the warm bath as an adjuvant to the taxis is spoken of as "often efficient" and proper to be employed; by some, local heat has been recommended. The bath is of service only by the general relaxation induced, and heat locally applied seems to us to be decidedly improper. It cannot affect the size of inextensible white-fibrous rings, and must increase the amount of blood brought into the constricted portion of the bowel, *i. e.* in one respect has no action favourable or unfavourable, in another tends to increase the

already existing difficulty of effecting reduction. Cold, on the contrary, while it does not affect the inelastic tissues, lessens the amount of blood, and stimulates to contraction the muscular fibres of the bowel, thus diminishing the hernial contents. Philosophically, cold is *the* local adjuvant and practical results are in harmony with theory. No reference is made to the use of ether or rhigolene spray, which has been frequently and successfully employed; nor, to "subcutaneous aspiration," which has been highly lauded by the few who have tried it. Herniotomy without opening the sac is commended in proper cases, *i. e.* those

"In which the seat of constriction is external to the sac; but as this point cannot always be determined beforehand, this mode of operating should be tried in every instance in which the duration of strangulation and the urgency of the symptoms do not contra-indicate its employment. A safe rule is that given by Birkett—to try this method in those cases and those only in which the taxis would be deemed justifiable."

Chapter XLIV.: "Urinary Calculus." Under this general head are embraced the nature, varieties, symptoms, and treatment of stone: lithotomy and lithotrity of course being considered somewhat at length. A valuable practical suggestion is, in lithotomy, to insinuate the finger into the bladder above the staff, "between its concavity and the roof of the urethra," thus obviating the difficulty in children of reaching the bladder.

Chapter XLVI.: "Diseases of the Urethra and Urinary Fistula." In this chapter we notice that it is, as usual, recommended after external perineal urethrotomy to introduce a catheter, and retain it for a couple of days. It is, we believe, becoming more and more the American custom to dispense altogether with the catheter, the advantage of keeping which in the wound is questionable.

The remainder of the work is devoted to the various diseases and malformations of the genital organs, male and female.

We have thus passed in review the several chapters of "Ashhurst's Surgery." Does it present the pathological views of the day, and the practice most in accordance with those views, as confirmed by the experience of surgeons competent to observe and decide? Unquestionably, yes; with possibly a very few exceptions, and these exceptions have reference to questions still under discussion. For students it is, we believe, the best work we have, and general practitioners will find pleasure and profit in reading it. Its statistical tables make it a work to which the author's fellow-workers will gladly refer. Dr. Ashhurst has done a good work, and we thank him for it. True, it is to a large extent a compilation, but it is well compiled; and who in these days could write a valuable *original* work on the whole immense field of surgical principles and practice? When the second edition is brought out, we hope that the size of the volume will be lessened by the omission of a large proportion of the "533 wood engravings" with which it is "illustrated." Its value will not, we conceive, be impaired thereby. P. S. C.

ART. XVI.—*Transactions of the American Ophthalmological Society. Eighth Annual Meeting.* Newport, July, 1871.

THE American Ophthalmological Society, organized in 1864, has now entered on the ninth year of its existence. The first two meetings were held in New York, the session of 1864 took place in Boston, that of 1867 at Niagara, and the subsequent ones at Newport, R. I. A full report of the "Transactions" has been published by the Society every year, except the first of its existence; and these volumes, although differing in size, all contain able and interesting articles by the practitioners devoting themselves to this specialty in various parts of the country, and will bear comparison with the transactions of any similar body abroad.

The present volume opens with a tablet to the memory of "Albrecht von Graefe, whose life and work greatly helped to turn a doubtful specialty into a certain science."

This is followed by a "Report on the Progress of Ophthalmology," by Dr. B. JOY JEFFRIES, of Boston, which catalogues the various articles on Ophthalmology published during the past year, both here and abroad, mentioning most works by title, and giving an abstract of others which have greater claims to novelty.

Following this is a description of "An Apparatus for Cutting Microscopic Sections of Eyes," by EDWARD CURTIS, M.D., of New York. We have here an account of an elegant and useful piece of apparatus which enables Dr. Curtis often to obtain a section of an entire eye; and further, an admirable description of the various processes of hardening, staining, washing, and preserving tissue, necessary to the proper display of its physiological and morbid anatomy. Dr. Curtis prefers paraffine, thinned by kerosene (its natural solvent, first suggested by Dr. W. Thomson, of this city), to the usual method of imbedding in wax and oil. Dr. Curtis uses for staining his tissues, carmine ʒss, borax ʒij, water fʒiv; in this solution his sections are immersed for half an hour, or until they have become of a "deep purplish-crimson." The staining fluid is then poured off, and without any washing at all, a saturated solution of oxalic acid, in *water*, is placed in the dish and changes the colour of the sections to a bright scarlet. As soon as it has done so thoroughly, it is poured off, and a little fresh solution is added. In this the sections are allowed to steep till the colour is discharged from all but the cellular or protoplasmatic elements of the tissue. The sections are then washed with water to remove the excess of oxalic acid transferred to alcohol, to be finally mounted in the ordinary manner. The author thus avoids the great waste of alcohol which occurs in the ordinary method of using oxalic acid.

Dr. FRANCIS DELAFIELD, of New York, presented a paper on "General Syphilitic Inflammation of the Eye." Localized inflammation of the eye—as, for example, inflammations of the iris and ciliary body—are familiar to most practitioners of medicine; "in rare cases, however, the inflammation becomes general; iris, choroid, ciliary body, retina, sclera, and cornea become involved, and the production of new cells is so great that staphylomatous tumours of large size are formed." Dr. Delafield gives a minute description of two such cases, both occurring in young negroes, and quotes from ophthalmic literature a similar case of Hippel, and two mentioned by Arlt.

A paper on "The Halo round the Macula Lutea," by EDWARD G.

LORING, M.D., of New York, follows. It is well known to all who have given much time to the study of the ophthalmoscopic appearances of the eye-ground, that the macula lutea usually presents itself in the upright image as an irregular yellowish spot (often triangular), surrounded by a darkly pigmented brownish border; while the same region of the eye-ground, viewed with the inverted image, shows us an ovoid brownish spot—its long diameter horizontal, surrounded by a more or less brilliant silvery rim. To explain these appearances, Dr. Loring resorts to the following ingenious hypothesis and experiment. Starting with Shultze's diagram of the region of the yellow spot, he assumes a depression throughout its whole extent, and reasons as follows: "As you will see figured on the diagram, this whole region bears in its formation a strong resemblance to a shallow cup, of which the rim is represented by a convex, the bowl by a concave surface. If we look upon these curved surfaces as mirrors, they would each have their foci: one lying behind, the other in front, according to their respective degrees of curvature. And if light should be thrown perpendicularly against such a combination of curves, the apex of the outside rim, or convex surface, would, from well-known optical laws, appear illuminated; while the inside, or concave surface, would appear more or less in shadow. Thus we would have the effect of a darker centre surrounded by an illuminated edge." By an ingenious experiment with eye phantoms, Dr. Loring shows that in any concave metal mirror, a depression of $\frac{1}{100}$ of an inch is sufficient to produce such an appearance.

To the best of the knowledge of the reviewer, no such depression has ever been demonstrated in the retina, except in the fovea centralis, and this is too small to produce such a reflex. Dr. Loring, in his paper, fully recognizes the force of this objection; but after stating most truly the extreme difficulties which beset the anatomist in any attempt to study accurately the distribution and relation of the retinal elements at this point, asks: "Under these circumstances, may not the ophthalmoscope have a voice in explaining a phenomenon which is in strict accordance with physical laws, and which outside of the eye can only be so explained?"

The next article is by HENRY D. NOYES, M.D., of New York, and is entitled, "A Scheme to Aid in Examining and Recording Cases of Functional Trouble of the Eye." After giving his blank form and explaining its method of use, Dr. Noyes remarks as follows on the method of measuring abduction and adduction at the ordinary reading distance: "The Test Object for adductive power, which has in my practice displaced all others, is a white dot and a line painted on a black card. The dot and line are of this form ! . I prefer this to either the dot above, or to the dot with the line both above and below. The line, as I make it, enables the patient to tell whether the false and true images are really perpendicular. By having it only on one side and short, the prism being 10° or 12°, the images are always apart from each other, and the patient is not tempted to put forth efforts at fusion of the images, which I have noticed to occur when the prolonged line of one image overlaps the other. A white image on a black surface is more conspicuous than black on white; and when a red glass is put before one eye, the difference in the new images is instantly recognized.

Dr. O. F. WADSWORTH, of Boston, Mass., furnishes some remarks "On

the Tables given by Loring and Knapp to show the Displacement of the Retina in Ametropia." These tables being of practical use, in order to enable us to determine the elevation of various parts of the fundus in cases of intraocular tumours or of choked disk, as well as the amount of depression of the nerve in cases of glaucoma, Dr. Wadsworth has done the profession good service by analyzing the formulæ by which they were calculated, and showing that with the exception of a slight error in each table, the formulæ are really convertible, and "must, therefore, from the same premises give the same results."

The apparent discrepancy arises from the fact that Loring (following Mauthner) has calculated the total amount of displacement in certain degrees of ametropia, while Knapp shows the amount of displacement in degrees of ametropia which are corrected by a lens of given focal distance placed at the anterior principal focus of the eye. The anterior principal focus of the eye (both using Listing's schematic eye as a basis), is 20.29 millimetres (almost exactly $\frac{3}{4}''$), in front of the second nodal point; and we must, therefore, in cases of hypermetropia, subtract three-fourths from the numbers of the glasses given by Knapp; and in cases of myopia, add three-fourths to obtain the true degree of ametropia. The slight difference still remaining is explained by the fact that Loring estimated degrees of myopia from the first nodal point; and degrees of hypermetropia from the second: while Knapp estimated degrees of myopia and hypermetropia both from the second nodal point.

Dr. G. HAY, of Boston, describes a "Variety of Forms of Small Pencils of Astigmatic Rays." This short and purely mathematical paper is in itself so condensed that it is impossible to present our readers with any satisfactory abstract of it, and we must, therefore, refer those interested in the subject matter to the original.

Dr. RUSSEL MURDOCH, of Baltimore, Md., calls attention to "The Retina an Asymmetrical Surface." In this somewhat lengthy paper, the author endeavours to show that astigmatism due to want of symmetry of the cornea may be compensated by a corresponding but opposite want of symmetry in the retina, "by the opposition of a concave cylindrical element to a convex" one.

To this view of the subject, Dr. Hay took exception at the meeting, and at the request of the Society, his views were committed to writing, and communicated to Dr. Murdoch, who was not present at the meeting, his paper having been read by Dr. Agnew. Dr. Hay's objection is essentially as follows:—

"According to my notion of the matter, if an astigmatism existed in consequence of an asymmetry of the cornea, then a pencil of rays proceeding from a *single point* of the object, say the point looked at, would, when arrived in the vitreous, be an astigmatic pencil. From such an astigmatic pencil which has no point focus, the retina could not receive a point impression, but only an impression extending over a certain surface, or a linear-shaped impression; and this although the pencil proceded from a single point of the object. This being the case, even if the retina became asymmetrical, it could not reduce to a point impression the surface impression, or the line impression made by the astigmatic pencil; that is, it could not correct the astigmatism. The impression on the retina would not be materially altered by the proposed change of shape of the retina."

Dr. GEO. STRAWBRIDGE, of Philadelphia, describes "An additional

Method for the Determination of Astigmatism." Dr. Strawbridge, fully recognizing the admitted fact that a minute point of light is the most delicate test for astigmatism, and having found (as have also previously Green and Thomson) that an illuminated bar is a more sensitive test than the usual black bar upon a white ground, has given us a useful additional method of determining astigmatism. This is based on the well-known experiment described at page 101 of Helmholtz Physiologische Optik, Leipzig, 1867, and on the additional remarks on the same subject, at pp. 138 and 139.

Dr. S. describes his method as follows:—

"In the centre of a Bristol board, a round aperture of thirteen millimetres in diameter is cut, and at a distance from the aperture of six centimetres, radiate bars cut in the Bristol board, having a length of nine centimetres, and width of five millimetres, and forming an angle with each other of ten degrees. Over this figure white gauze paper is passed, a lamp placed behind illuminates it in its entire extent. The patient is placed at a distance of 20′ from the figure, and requested to observe the round central opening, and to notice in what direction it is most elongated. This is readily determined by observing to which of the bars in the figure the light prolongation most closely corresponds in its direction, and the result will be controlled as to accuracy, by seeing which of the bars are most distinctly seen. For example, in a case of myopic astigmatism in the vertical meridian, it would be found that the light elongation would be upward and downward, and at the same time the vertical bar would be most distinctly seen.

"By this procedure the direction of the vertical meridian is discovered. The next step is to determine the refraction of this meridian. To this end a diaphragm is advanced in the direction of the greatest elongation of the round light (suppose it to be vertical, and that the diaphragm moves from above downward), and the patient is requested to notice whether the upper half of the round light first disappears, or the lower half; if the upper half is first gone, the meridian is shown to be so curved as to cause a myopia to exist; while if the lower half of the round light is first to disappear, we conclude that a hypermetropia exists. If the entire round light is found to disappear at once, it may be concluded that very little astigmatism exists. The direction of the meridians being now known, as well as their refraction, whether normal or so curved as to cause a myopia or hypermetropia, the next step would be to determine exactly the amount of abnormality. To this end we proceed with spherical glasses, determining the exact one necessary to see distinctly the proper bar, as in the method laid down by Snellen."

John Green, M.D., of St. Louis, Mo., calls attention to "Astigmatism as an active cause of Myopia," and the great frequency of astigmatism in myopes. Analyzing with this view the tables previously published by Snellen and himself, Dr. Green argues that astigmatism is a frequent and active cause of myopia, because, owing to the indistinct image formed in the retina, there is a tendency to approach the book so as to obtain larger images, and thus read with high degrees of convergence, and with a strong exercise of the accommodation.

"Remarks on Cataract" is the title of a paper by E. G. Loring, M.D., of New York. Recognizing the frequency of astigmatism after operations for cataract, and having found that the necessary sphero-cylindric glass is "so heavy, and of so awkward a shape as only to be worn with great discomfort by the patient," Dr. L. proposes to obtain its advantages in a lighter and more comely form, by grinding the spherical glass plano-convex, and by cementing to its centre, by Canada balsam, a smaller cylindroid glass of a diameter corresponding to the smaller diameter of an ovoid spectacle frame. According to Dr. L.'s experience, such

glasses bear any reasonable amount of rough treatment without coming apart.

The second part of the paper is devoted to the statistics of cataract operations, and shows by a comparison of the statistics of the flap and peripheric linear operation, that the percentage of success in each is more nearly balanced than most are disposed to admit.

We quite agree with Dr. Loring in thinking the standard now used by most operators, including $V = \frac{1}{10}$, among the "perfect results," to be too low; moreover, until, in publishing cataract statistics, the acuity of vision in each case is determined separately (as given by the use of the Snellen tables), and all results from normal acuity of vision to $V = \frac{1}{10}$ are no longer lumped together as "perfect results," it will be almost impossible to derive any accurate information as to the experience of different operators, or as to the comparative merits of different methods of operating. We think, however, that Dr. L., in his estimate, entirely overlooks the great advantages offered by the peripheric linear section, owing to the diminished confinement to bed, and the shorter stay in hospital required by those who are operated on by this method, and also the lessened liability to accident (such as bursting open of the wound) on the part of restless or delirious patients, especially such as, not being operated on in hospital, are less under the control and supervision of the surgeon. Inasmuch, however, as we have given a comparative estimate of the advantages of the new and old operations, and also of the necessary length of after-treatment, together with the most noteworthy statistics on the subject, in the January number of this Journal for 1871, we deem it unnecessary to enlarge further on this point.

Dr. HENRY D. NOYES, of New York, describes "An Apparatus for testing the Perception of Colour." This consists of a convenient revolving disk, so arranged that only one colour is at a time presented to the eye. It is ingenious, and much shortens the time necessary for making such observations.

Dr. GEO. STRAWBRIDGE, of Philadelphia, describes a Jaeger Ophthalmoscope as modified by himself. The changes introduced by Dr. S. in this instrument, consist in replacing the fixed handle by a folding one, and by adding at the back of the instrument, three revolving disks, carrying twenty correction glasses, thus obviating the trouble of resorting to a test set for appropriate glasses, and rendering the instrument a more portable one. The disks are on the principle of those long since used by Rekoss of Königsberg, and more lately by Dr. Loring, of New York, in his admirable ophthalmoscope. To the latter, Dr. S. acknowledges his indebtedness for the idea. Like the original instrument of Jaeger, this modification has the advantage of the strong and weak light mirrors, also that the correction glasses remain stationary when the mirror is placed at any given angle, and thus avoid any distortion of the images by prismatic action.

W. W. KEEN, M.D., and WM. THOMSON, M.D., relate a case of "Gunshot Wound of the Brain followed by Fungus Cerebri and Recovery, with Hemiopsia." The extremely interesting case whose clinical history is here carefully detailed, is particularly interesting from the extremely minute investigation of the field of vision. The patient possessed full acuity of vision, and was very intelligent, and the authors believe that they have proved that "at the *point of fixation*, the vertical line which bisected the field seemed to deviate slightly to the defective

side." If this be granted, then not only is the yellow spot the optical centre of the eye, but it alone is supplied throughout its entire extent by a double set of nerve fibres, one coming from each cerebral hemisphere.

H. D. NOYES, of New York, gives an account of a case of "Detachment of the Retina with Laceration at the Macula Lutea." This remarkable lesion was examined conjointly both by Drs. Noyes and Delafield, and both agreed as to the interpretation of the appearances in the region of the yellow spot. Dr. N. remarks that it differed from any other retinal rent that he had seen, in its being round and with smooth edges, as if "it might have been cut out with scissors or made with a punch." The eye had been struck by the cork of an ale bottle three years previously, and the sight was lost immediately. Externally it appeared normal, except a slight divergence. There had been no subsequent inflammation, and at date of examination only an obtuse perception of light.

Dr. B. JOY JEFFRIES, of Boston, gives a "Report of Passavant Operations with and without Ether, and also under Nitrous Oxide." The author last year communicated to the Society a report of thirteen operations for breaking up posterior synechiæ after the manner proposed by Dr. Passavant. He reports now an additional series of seven cases, and says "that in these seven cases, as in the thirteen operations previously reported, I did no harm to the capsule and certainly improved the condition of the eye." "Instead of an iridectomy knife, I now use a broad paracentesis needle. I find no difficulty in manipulating my delicate iris forceps in the corneal wound this makes, and I lose but little aqueous before the iris is grasped, when the escape of fluid rather assists in breaking the attachment. Atropia is continued, and the humour secretes so quickly that there is no time for the iris to fasten itself again to the capsule. I have occasionally succeeded in not losing all the aqueous humour during the operation."

Dr. Jeffries has found nitrous oxide preferable to any other anæsthetic for this or any "short" operation.

Dr. JOHN GREEN, of St. Louis, Mo., describes a case of "Readjustment of the Levator Muscle of the Upper Lid." The subject of it was a boy five years of age, who was gored by a bull, and the whole upper eyelid torn from its attachment, with the exception of a narrow bridge at its outer end. When seen by Dr. G. two years subsequently, there was accurate union of the skin and complete ptosis. The eye itself was wholly uninjured. Believing the case to be one of complete detachment of the tendon of the levator, Dr. Green decided to operate, and proceeded as follows: "An incision of rather more than an inch in length was made through the skin along the upper border of the lid, and the dissection extended through the subcutaneous fatty tissue in a direction nearly backward, so as to avoid opening the conjunctival sac. The upper surface of the eyeball was thus reached without having encountered any fibres of the orbicularis muscle, or in fact any other tissue than subcutaneous fat, which was apparently continuous with the fat of the orbit. A broad, thin tendon was seen spread out over the exposed part of the globe, which proved to be the tendon of the superior rectus. Between this and the roof of the orbit the tendon of the levator was easily discovered, and no difficulty experienced in uniting it by three sutures to the front of the tarsal cartilage. The wound was then closed by three other sutures, leaving the ends of the three deep sutures protruding externally. The subsequent swelling was moderate, the external wound healed promptly without sup-

puration. The superficial sutures were removed on the fourth day, and the deep sutures left to become detached spontaneously."

Two months later the boy could open and shut his eye simultaneously with the other.

The same author gives an "Additional Note upon the Use of Atropia in the Treatment of Incipient Strabismus." Dr. Green here refers to his paper on this subject in the "Transactions of 1870," and gives his conclusions based on another year's experience. They are: 1st. That in commencing squint paralysis of the accommodation by atropia "leads to a speedy abandonment of the habit of squinting. 2d. That the artificial amblyopia thus produced supplies a needed motive for accepting convex glasses. 3d. That by repeated instillations the habit may be effectually broken up and the case at last brought under the control of convex glasses; the length of treatment necessary to obtain this result varying from one month to upwards of a year, according to the previous duration of the squint. 4th. That such results have been obtained in cases of periodic squint of fully three years' standing. 5th. That in cases of permanent deviation where the visual axes, even in a state of rest, do not become parallel, the position of the eyes is nevertheless improved, and that we have, in the degree of remaining squint, an indication of the extent of the correction to be sought from tenotomy.

The three last papers are entitled: "Paralysis of the Trigeminus, followed by Sloughing of the Cornea," by Wm. F. Norris, M.D., of Philada. (*Vide* January number of this Journal.)

"A Case of Congenital Fissure of the Lids," by W. W. Seely, M.D., of Cincinnati.

"Congenital Absence of both Eyeballs," two Cases, by Dr. George Strawbridge, of Philadelphia.

For a detailed account of the "Freaks of Development," described in the last two papers, we must refer our readers to the articles themselves.

W. F. N.

ART. XVII.—*A Practical Treatise on Diseases of Women.* By T. GAILLARD THOMAS, M.D., Professor of Obstetrics and Diseases of Women and Children in the College of Physicians and Surgeons, New York; Attending Surgeon to the New York State Woman's Hospital, &c. &c. Third Edition, enlarged and thoroughly revised, with two hundred and forty-six illustrations on wood. Philadelphia: Henry C. Lea, 1872.

SIR Henry Holland, whose recent work[1] all doctors can make themselves wiser and better by reading, remarks that in his several visits to the United States, he has been consulted by patients to whom it was a pleasant office to render aid in return for the many kindnesses he had received in this country; but that really such aid was little needed "where both the principles and practice of medicine are derived from schools of instruction, and a medical literature in no wise inferior to our own." Such a tribute to American medical colleges and American medical literature, coming from one whose intelligence, wisdom, honesty, and opportunity cannot be doubted, is indeed worthy of grateful appreciation by all American physicians.

[1] Recollections of Past Life.

We could not but be struck with the justice of Dr. Holland's statement so far as it relates to one department of our literature, in examining the third edition of Professor Thomas's well-known work on *Diseases of Women*. Its able author need not fear comparison between it and any similar work in the English language; nay more, as a text-book for students and as a guide for practitioners, we believe it is unequalled. In the libraries of reading physicians we meet with it oftener than any other treatise on diseases of women, and the demand for it, which has rendered necessary the issuing of a third edition within four years after the publication of the first, testifies the general approval of it by the profession.

We do not propose an extended review of this volume; when the first edition appeared, its general features and character were presented[1] in detail; but the work has undergone numerous changes, both alterations and additions, and is very far from being a reprint, and therefore demands a fuller notice than would otherwise be proper; besides, we wish to qualify and discriminate somewhat in the commendation justly due it.

The first chapter, *Historical Sketch of Gynæcology*, is somewhat fuller than in previous editions, while the second, *The Etiology of Uterine Diseases* is scarcely altered; the third, *Diagnosis of the Diseases of the Female Genital Organs*, like the first, has undergone both increase and improvement: especially do we wish to commend in it the rules given as to the use of *tents*, rules for preventing, so far as human foresight can prevent, the mischievous consequences which may result from these oftentimes valuable means of diagnosis. Among the specula described, we miss any reference to Dr. Meadows's instrument, which is certainly compact, portable, neat, convenient, and in some cases will be found quite useful.

The fourth chapter, *Diseases of the Vulva*, is but slightly altered; a valuable addition, however, has been made to it in a consideration of *hydrocele*.

Dr. Thomas asserts, in accordance with the statement of most authorities, that the fibres of the vaginal sphincter inosculate with those of the anal sphincter, making a figure 8, an assertion which is not in accordance with the teaching of Dr. Savage.[2]

Under the head of *inflammation of the vulvo-vaginal gland*, the usual duration of the disease is said to be from two to three weeks. With all deference to Dr. Thomas's large experience and accuracy of knowledge, we believe that suppuration, which is the rule in the great majority of cases where the symptoms are so urgent that professional aid is sought, generally occurs within a week, or ten days at most.

In considering *pudendal hemorrhage*, Dr. Thomas gives a plan of treatment, and adds, "It is difficult to conceive of any case occurring in the non-pregnant woman, which could resist this method if effectually employed." Certainly the difficulty of the conception must be obvious.

Pruritus vulvæ is well discussed, both in its etiology and therapeutics. There are two plans of treatment, however, directed to the pruritus especially, which we have sometimes found so useful that we are constrained to mention them: the one is by fomentation with hot water—as hot as the patient can bear it; the other is by applying patent lint, dipped in a solution[3] containing ten grains of carbolic acid, eight of

[1] American Journal of Medical Sciences, July, 1868.

[2] Anatomy of the Female Pelvic Organs.

[3] The formula is taken from Dr. Atthill's Lectures on Diseases of Women, Dublin, 1871.

acetate of morphia, two drachms of dilute hydrocyanic acid, four drachms of glycerine, adding sufficient water to make four ounces.

Rupture of the perineum is the subject of the fifth chapter. Dr. Thomas states that all cases of this accident may be classed under four heads:—

"1st, superficial rupture of the fourchette and perineum, not involving the sphincters; 2d, rupture to the sphincter ani; 3d, rupture through the sphincter ani; 4th, rupture through the sphincter ani, and involving the recto-vaginal septum." Where would *central* rupture be placed? We much prefer dividing these accidents as follows: *partial*, *complete*, *central*, and *complicated*. A simple rupture of the fourchette—or *posterior commissure*, we would prefer calling it, and banishing the former term—need not give the accoucheur or the surgeon any special anxiety, and is hardly worthy of consideration.

Three circumstances are mentioned as militating against the success of *immediate* operations for ruptured perineum, viz., the inexperience of the operator, consequent hurry, &c., the contact of the lochial discharge, or of dribbling urine, with the surfaces which are brought in apposition. But to our mind, the most important one is omitted, viz., the *bruised* condition of the tissues and the lacerated wound.

Vaginismus, chapter sixth, has two more pages than in the second edition. Dr. Thomas states, in reference to Sims's operation, that there is a growing distrust in the minds of many gynæcologists as to the necessity of resorting to a procedure which is reported in one case to have resulted in fatal hemorrhage. He also quotes Scanzoni and Tilt as condemning the surgical treatment of this affection. The author still holds to his recommendation of *ethereal copulation* in certain cases. Now, we should be glad to know how many vaginismic wives have been cured, how many ether-begotten children have been born, in the four years this recommendation has been before the profession. Surely *mechanical impregnation* might be advocated on precisely the same ground as anæsthetizing a wife in the hope that complete connection, then accomplished, "may result in pregnancy"! A small uterine syringe could be readily introduced into a vagina whose portals would be closed to a larger body, and its homuncular contents, legitimately obtained in a way more easily imagined than described, discharged into the uterine cavity; though this sort of man-planting might lead some new Uncle Toby to ask, in his bachelor innocence, are children not born, but begotten with a squirt?

The seventh chapter, which is upon *vaginitis*, calls for no special remarks, except that we hope no one will imitate Dr. Thomas in using the word *acuity;* according to our American authority the word is dead, and we are sorry to see that so excellent and accurate a writer as Dr. Thomas is trying to resuscitate it. Another word, by the way, which is a favourite with Dr. Thomas, but which to our mind is exceedingly unpleasant, is *fornix*.[1] Why not plainly say arch or roof; and if necessary to specify a particular part of this vaginal roof, describe as anterior, posterior, right lateral, or left lateral cul-de-sac?

[1] It should be borne in mind that this word also means a *brothel*—It is thus used by many of the ancient authors, Juvenal and Horace among others—and from it we have fornication, &c. We believe its use signifying the vaginal roof unnecessary if not unfit—we hope we shall not again meet with vaginal brothel in any of our professional classics.

Atresia vaginæ, the subject of the eighth chapter, is excellently considered; so too are *Prolapsus Vaginæ* and *Vaginal Herniæ* in the ninth chapter.

The tenth chapter is upon *Fistulæ of the Female Genital Organs*. In the author's description of the operation for vesico-vaginal fistula, we do not think sufficient prominence is given to scissors as every way preferable to the knife for denuding the fistulous margins; true, we are treated to a cut, fig. 40, of scissors that, it is said, may be used for this purpose, but the same scissors make their appearance, fig. 220, for amputation of the neck of the womb! We should be very glad to see Dr. Emmet's two forms of scissors represented. We do not believe Dr. Thomas, with all his genius and skill, and he has such most richly granted him, would attempt the operation with the scissors which he has delineated on page 184. So, too, Emmet's *conical* needle is, in our opinion, much better than any of which representations are given.

Under the head of *urinary fistulæ requiring special treatment*, the author considers, among other forms, *uretero-vaginal fistula*, but merely as "the condition" remaining after an operation for vesico-vaginal fistula, performed at Hôtel-Dieu by our distinguished countryman, Dr. Bozeman: the case is unnecessarily detailed, and not a word exists as to any other examples of this lesion existing independently of a vesico-vaginal fistula, cured or uncured, nor a word as to its treatment. Nearly five years ago the writer published a case[1] of this injury manifested after parturition, and it, the sole and primary lesion, after having existed for sixteen years, was successfully operated on.

Chapter twelfth, *General Considerations upon Uterine Pathology and Treatment*, is at least a third longer than the corresponding chapter in a previous edition: it is the best and clearest discussion of vexed topics, in so brief a space, that we have ever read.

With the thirteenth chapter the author enters upon the consideration of uterine diseases proper; but at the twenty-fourth chapter—prior to the consideration of the disorders of menstruation—the author *goes off*, shall we say?, to discourse on *perimetritis* in its different forms; viz.,

[1] This case was reported in the *Western Journal of Medicine*, October, 1867, and was translated and republished in the *Medicinisches Correspondenz-Blatt des Würtembergischen arztlichen Vereins*. Undoubtedly, it was the first case of the kind ever reported in this country—probably the first ever reported anywhere. Dr. Freund, of Breslau, states that but three cases of this kind have ever been observed. (*Journal of the Boston Gynæcological Society*, January, 1872.) Deroubaix, in his works upon *genito-urinary fistulæ*, Brussels, 1870, gives the details of a post-mortem of a patient dying at forty-one years, who had a congenital uretero-vaginal fistula: but of course this does not militate against our statement as to the case under our charge; to wit, that this uretero-vaginal fistula was the first recorded where this was the *sole* and *primary* lesion.

Simon has met with this accident once or twice; and so unsatisfactory, we presume, was his treatment, he makes uretero-vaginal fistula one of his eight indications for *kolpokleisis*. Our impression is, but of this we will by no means be positive, that Simon's first published reference to this form of fistula was in 1868.

Duclout, writing in 1869, *Gazette Médicale*, p. 170, recommends essentially the very plan which we had successfully executed two years before.

The writer does not claim to be that most troublesome and unhappy of creatures, the man with a grievance; nevertheless, it seemed to be due himself, as well as due professional science, when the leading American author on diseases of women ignored uretero-vaginal fistula except as an occasional consequence of an operation for vesico-vaginal fistula, to point out the error.

pelvic cellulitis and *pelvic peritonitis*, and one of the occasional consequences, viz., *pelvic abscess:* then *pelvic hæmatocele*, which is one of the causes of pelvic peritonitis, is discussed.

In the twenty-eighth chapter he returns to uterine diseases, and we have chapters successively devoted to *uterine fibroids*,[1] *fibro-cysts*, *polypi*, *cancer*, *cancroid*, *cancer of the body of the uterus*, *recurrent fibroid;* then a chapter in which *uterine moles*, *uterine hydatids*, *superinvolution*, and *subinvolution* of the uterus are presented, a single chapter for these very dissimilar conditions, while there are three chapters for the discussion of cancer—which, in its different locations and forms, yet has one common tendency. Then the author gives us disorders of menstruation, metrorrhagia, sterility, and leucorrhœa under a general heading, "*On some of the most important results of uterine disease.*"

Now, so far as it concerns disorders of menstruation, are they not, at the starting-point, frequently the *causes* of uterine disease, though afterwards the uterine disease may complicate and perpetuate them? And is not a pathology which founds menstrual derangements upon uterine diseases in danger of resulting in therapeutics that might be too exclusively local, on the part of some practitioners at least? Possibly, most physicians would assert that they find very many, if not a majority, of the cases of menstrual disorder they are called to treat, dependent upon constitutional condition, upon ovarian disease, &c., rather than upon uterine.

We desire also to say a word as to Dr. Thomas's classification. Richter, we believe, has declared every man has a right to his own style of composition, as much as he has to his own nose; and so we judge our author, whose rare abilities we sufficiently admire, may classify uterine diseases according to such plan as he thinks best. But is it the best—the most philosophic, and fixing itself most readily and permanently in the mind of the student? We certainly think not; however, it would require many details, occupying too much space, to give our reasons for this negative opinion. We beg those who think "the king can do no wrong," to contrast Dr. Thomas's classification with that of the late Dr. H. G. Wright;[2] Dr. Wright divides uterine disorders into those of *place*, *function*, and *structure;* those of the latter being subdivided into *cacoplasia*, *heteroplasia*, *hyperplasia*, and *aplasia*.

Returning from this digression, we shall make but two or three remarks upon the chapters included between the thirteenth and the twenty-fourth. The author has substituted *areolar hyperplasia* for *chronic corporeal metritis*, and the chapter devoted to its discussion is one of the longest and most valuable in the volume.

In discussing *versions of the uterus*, we are shown a new pessary of his own, and also a modification of Cutter's pessary—certainly the modification is a great improvement. Dr. Thomas speaks very highly of Dr.

[1] In the chapter devoted to the consideration of those tumours hitherto known as uterine fibroids, Dr. Thomas uses the term *myo-fibromata*, asserting that neither *fibroid* nor *myoma* expresses their true character, but that being composed of both muscular and fibrous tissue, the designation which he has given is the correct one. Will he allow us to call his attention to the article *Fibrome ou Tumeurs Fibreuses*, by Heurtaux (*Nouveau Dictionnaire de Médecine et de Chirurgie Pratiques*, tome quatorzième, p. 675), in which, it seems to us, the argument in favour of *myomata* as the proper term for these uterine tumours, is conclusive.

[2] Uterine Disorders. London, 1867.

Albert H. Smith's pessary. Judging from the wood-cut which is given, this pessary differs very little, if at all, from one of the many forms of pessary used by the late Sir James Simpson.

Under the head of *inversion of the uterus*, the author discusses *abdominal section as a substitute for amputation*, reporting two cases where such section was made, and the inversion, which had baffled all other means, readily reduced after intra-abdominal dilatation of the cervical ring. One patient recovered entirely, in a year became pregnant, and in the eighth month of pregnancy died suddenly with symptoms of cholera morbus, after eating some canned oysters; the other patient did well for forty-eight hours, and then peritonitis occurred, rapidly proving fatal.

Chapters upon the *Diseases of the Ovaries*, &c., and of the *Fallopian Tubes* follow those on uterine diseases, while the volume concludes with a chapter upon *chlorosis*.

Of course *ovariotomy* is fully considered, both as to the statistics, and as to the method of its performance.

Dr. Thomas, in referring to the former, remarks: "The time has passed when in an essay upon this subject the question need be discussed as to the propriety of recognizing ovariotomy as a legitimate resource in surgery." What a contrast between these words and those uttered only twelve years ago by one of the most celebrated and able of American physicians, Dr. Charles A. Lee, whose recent death we greatly deplore! "On the whole the conclusion appears to us irresistible, that the extirpation of an ovarian cyst is, in every view of the subject, unjustifiable, and should be abandoned."[1]

American surgeons have contributed to establish ovariotomy upon a firm basis of professional and popular confidence. Not only is the operation American by virtue of its having been first performed by Dr. Ephraim McDowell, of Kentucky, but to-day the two operators who have had the largest percentage of successful cases are Americans—the late Dr. Bradford, of Kentucky, and Dr. Marion Sims, the former having twenty-eight recoveries in thirty-one operations; the latter ten recoveries in eleven.

Nevertheless, such success is so far above the average, that it ought not to be taken as the basis for the probabilities of cure in a given case, nor as the term of comparision by which individual operations are to be judged.

As to the period at which the operation should be undertaken, the author lays down the following general rule: "If a small cyst be discovered which is removable by the vagina, it should be removed as soon as possible, while one too large for this should be interfered with when it is evident that the patient is failing in strength, and becoming emaciated, depressed, and nervous."

Dr. Thomas states that during the last eight months he has performed ovariotomy eight times, with six recoveries and two deaths. Dr. Atthill remarks, *op. cit.*, p. 135, in reference to this operation, "still making every allowance for improved diagnosis, and for greater care in the selection of cases, I do not think we can hope to raise the percentage of recoveries permanently above sixty-five per cent." Mr. Hutchinson[2] thinks that the average mortality in most operations is not less than fifty per cent.; that

[1] Copland's Dictionary (American edition), vol. i. p. 749.
[2] Holmes's System of Surgery, vol. v. p. 62.

in practised hands two recoveries to one death is a proportion which may be confidently expected, and that perhaps we may even very considerably improve on this. It will be observed that Dr. Thomas's recent operations show the very satisfactory result of three recoveries to one death. One of the most important questions in this operation is in reference to the treatment of the pedicle, and no less than eleven[1] different methods are mentioned, the last being Dr. Emmet's, viz., using silver wire passed repeatedly through the pedicle by the cobbler's stitch. Few men can manipulate silver wire and needles with the facility and celerity of Dr. Emmet; certainly most operators will find double-stitching with silver wire a broad, thick pedicle somewhat tedious if not difficult—we have tried it twice, and we are satisfied that it is so.

Dr. Thomas narrates an interesting case of *vaginal ovariotomy* successfully performed by himself;[2] it being the only case he has met with in which he felt justified in resorting to this procedure. In speaking of the operation, he says, that "in certain rare cases in which a tumour not larger than the head of a child a year old falls down into Douglas's *cul-de-sac*, it will be possible to cut through the vagina, seize the sac, draw it down, ligate the pedicle, and return the stump to the abdomen. If this can be done, a great deal of risk will be avoided, and the patient spared a lengthy period of suspense, with the prospect of a serious capital operation at the end."

Dr. Thomas does not believe that the scope of this plan of performing ovariotomy will ever be very great, but thinks that in cysts of small size, which are unattached, it will offer a valuable resource for the avoidance of years of mental suffering while the disease is progressing, and of the capital operation of abdominal ovariotomy in the end with all its attendant dangers and uncertainties.

One chapter we hoped to see in this edition, but are disappointed; and the disappointment is not ours only, but will be shared by the hundreds of physicians who read this book—a chapter treating of *diseases of the female bladder and urethra* would be prized most highly, and is greatly needed. How happens it such diseases are utterly neglected, or most briefly and unsatisfactorily considered, in our standard works on diseases of women?

A well-prepared monograph upon these maladies undoubtedly would meet with great success.

We conclude our brief review by repeating the hearty commendation of this volume given when we commenced: if either student or practitioner can get but one book on diseases of women, that book should be "Thomas." T. P.

[1] A twelfth might have been brought forward—dividing the pedicle by twisting with strong clamps, or forceps, the torsion of the vessels thus made securing against hemorrhage, a plan which has been devised and successfully executed by an eminent Glasgow surgeon.

[2] For report of this case, see No. of this Journal for April, 1870, page 387.

ANALYTICAL AND BIBLIOGRAPHICAL NOTICES.

Art. XVIII.—*First Biennial Report of the Board of State Commissioners of Public Charities of the State of Illinois.* Presented to the Governor, Dec. 1870.

The Legislature of Illinois, during the session preceding the presentation of this Report, voted an appropriation for largely increasing the means of accommodation for the insane. Desirous of ascertaining and adopting the best plan for the management of this class, the Board called a conference of State officers, officers and trustees of insane asylums, and others, to discuss the whole subject. At this meeting letters were read from most of the superintendents of insane hospitals in this country, responding to questions previously addressed them, as to the comparative merits of the so-called "congregate" and "segregate" systems. While the writers generally distrusted and disapproved the latter and newer plan, most of the gentlemen present at the conference approved and desired to have it tried.

Without designing to follow the discussion on that particular occasion, it may not be unprofitable to examine into the claims somewhat confidently put forth by the advocates of a new method of maintaining and caring for the insane.

During the last ten or twelve years there has appeared, in various directions, a spirit of dissatisfaction with the manner in which our insane are managed. We do not now refer to the vulgar suspicion and distrust which fancies every hospital a "Bastile,"—to use the pet word of the sensation reporters. We refer to a feeling among cultivated people, philanthropists, officers of State governments, and even among physicians, that the prevailing method of massing together the curable and incurable, quiet and boisterous, harmless and dangerous, idle and industrious, in one large building, is not the best one for the interest either of States or of patients. Two points especially are insisted on by the would-be reformers: First, the alleged excessive and needless cost of the great asylums, in erection and in maintenance; Second, the restriction of the patient's personal liberty, to a degree not required by his condition, and consequently, just so far a violation of his rights, and often an obstacle to his recovery or improvement.

To remedy these alleged evils various plans have been proposed. In nearly all of these the germinal idea has been derived from the sort of family-life of the insane with their keepers, in the ancient Belgian colony of Gheel. It will, therefore, be proper to glance at the facts as to the condition of the insane at this place. The history, and the more obvious outward aspects, of the village are familiar to all, from the descriptions of countless tourists. Notwithstanding the generally laudatory and rose-coloured views given us by curious and unprofessional travellers, scarcely any one, at least among those practically acquainted with insanity, now believes an American or an English Gheel to be practicable, whether desirable or not. Until within a few years the published accounts of Gheel have been the superficial impressions of unscientific tourists,

more desirous to tell a marvellous story than to ascertain and present the true merits of the system. The facts that the cases there treated were carefully selected ones, and that even of these the condition and treatment were by no means altogether satisfactory, were totally ignored. During the last ten or fifteen years, however, Gheel has been examined by many English and American alienists, bringing to their observation a thorough, practical acquaintance with the insane, and a sincere desire to ascertain the real value of the system. Drs. Henry Stevens, of St. Luke's, London, and John Sibbald, of the Royal Edinburgh Asylum, made their visits to Gheel the subject of articles in the *Journal of Mental Science*.[1] Dr. J. B. Tuke also, in a paper in the same journal[2] on "The Cottage System of Management of Lunatics as practised in Scotland; and Suggestions for its Elaboration and Improvement," incidentally speaks very strongly of his personal knowledge of the faults and imperfections of the Gheel colony. Several gentlemen from this country, prominent in asylum management, including Dr. John E. Tyler, late of the McLean Asylum, and Dr. Pliny Earle, of the Northampton State Lunatic Asylum, have borne strong testimony to the wretched and inadequate character of the provision there made for the patients.[3] Whatever may have been the fact years ago, at present, Dr. Tuke tells us, the rules of the colony exclude all "suicidal, homicidal, and incendiary lunatics," and "those who require continual restraint or coercion;" also "those *whose escapes shall be frequent*, or whose disease infringes public peace or decency." Even with this selection of cases, out of eight hundred patients, one hundred are, of late years at least, confined in a central hospital of the ordinary congregate sort; and from fifty to seventy others are subjected to mechanical restraint—principally by "hobbling," as is done with cattle to prevent too free a use of the legs. The other gentlemen named, visiting the colony independently and at intervals of some years, report essentially the same facts. Dr. Stevens and Dr. Sibbald distinctly intimate that the Gheel lunatics are not prevented from indulging in filthy and vicious habits to an extent impossible in a well-ordered hospital. The latter points out the absence of the means necessary to secure cleanliness of persons, clothes, and bedding. The former noticed that the liberty allowed certain demented patients was abused in a manner impossible under asylum supervision. Dr. C. L. Robertson, of the Sussex Lunatic Asylum, speaks of an English Gheel, as "utterly impracticable, and if practicable not very wise."[4]

It may be not unreasonably asked, why, after admitting that scarce any reformer desires to try precisely the Gheel experiment in this country, we yet spend time in exhibiting the faults of that plan? We do this for a twofold reason: the men who have so clearly seen and stated the facts about Gheel, are the very ones most enthusiastic in the advocacy of various adaptations, alterations, or modifications of the same fundamental idea; and while agreeing together to condemn the original, disagree entirely as to the desirable method of improving upon it—even bringing against each other's plans criticisms nearly as severe as those upon Gheel. It seems to us that this union in adverse judgment upon a system long established and well known, taken in connection with this disagreement as to plans as yet theoretical, or but imperfectly tried, is very significant. It leads us to anticipate, what we think general experience, *à priori* reasoning, and experiments so far made, all tend to indicate—viz., that for the

[1] Vol. iv., 1858, and vii., 1861. [2] Jan. 1870, vol. xv.

[3] See Earle's Address on Treatment of Insane Poor in U. S., before Mass. Med. Soc., June 2, 1868.

[4] Vol. x. Journ. Men. Science, Jan. 1865—"On the Means of Providing," &c.

proper remedial and custodial care of the insane, no practicable form of the "cottage," "family," or "segregate" system can ever be largely available. That a certain extremely small class of patients might possibly be as well and happy in a "cottage" we would not absolutely deny. That they would be as well cared for, we do not believe.

Before setting forth at length the details of the plan now proposed for adoption in this country under the general name of the "American Cottage System," let us glance at experiments already made and making in Great Britain, with a view to seeing how far these have succeeded in promoting the welfare of the insane and reducing the cost of maintenance. These attempts, under the titles of "family" and "cottage," are, it is to be remembered, modifications of the Gheel plan.

Dr. Tuke, in the essay before quoted, has severely criticized the Scottish cottage system as exemplified in the district adjoining his asylum. Here the patients are placed singly or in twos, threes, and fours, in the families of poor peasants, villagers, and tradesmen. Far from all enjoying the delights of rural life, many of them are in the back streets of the towns. Some of the cottages, he says, should more properly be called hovels. Some were filthy, most of them damp and uncomfortable. The inmates were almost exclusively demented and idiotic. There were no facilities for bathing. The food was poor and insufficient. Some of the keepers seemed kind and careful, others low and brutal. The patients who had been brought from the asylum did not appear more contented. Some expressed their sense of *ennui*, and longed after the amusements of asylum-life. Others felt degraded at being controlled by persons of their own or a lower class. "They did not seem to feel themselves free at all." Very little effort seemed to be made to employ them in any way. Cleanliness was sadly neglected. Dr. Tuke remarks upon the comparative comfort enjoyed by patients during bad weather, respectively, in "the stifling kitchen of a hovel," and in the airy wards of his own neighbouring hospital. He moreover goes so far as to make this remark—"In my own asylum any one of these patients would have had greater liberty of action, less restraint, and better and freer air." Admitting a saving of about one-third in cost of maintenance in this particular district, he adds that the saving is made at the expense of inadequate provision for the comfort of the patients. He also makes the pertinent observation that the greater part of the pauper insane were not paupers before their insanity, and that consequently their treatment upon a purely pauper basis is little short of cruelty. The somewhat unpleasant thought is suggested, that whenever the peasant-alienist finds his finances straitened, he will be likely to begin his retrenchments by starving his boarders.

Is it not now rather surprising to find that this very gentleman believes in the practicability and wisdom of this very cottage or family system? He thinks it needs only the addition of a simple machinery of inspection to make it better than the congregate system. After so fully stating the evils of the Scottish method as it exists, and moreover admitting the very small proportion of cases fitted for the most perfect cottage-system, and the small amount of tact or discretion to be expected from an ignorant peasantry, he yet proposes to render the system a success by gathering all the cottages into one neighbourhood, subjecting them to the visitation of a medical superintendent, who shall be the head of the county asylum, and to the control of the Lunacy Commissioners of the district. Still, he would place in them only the demented and idiotic.

Dr. C. L. Robertson, in a number of papers published in the *Journal of Mental Science*—from one of which we have already quoted his views concerning Gheel—upon the various plans for disposing of the insane, seems to favour a

system almost identical with that approved by Dr. Tuke. While violently opposing the Gheel arrangement, he professes warmly to approve the placing of patients, in twos or threes, in cottages upon the grounds connected with an asylum, and under the same medical supervision. To test the practicability of his views, Dr. R. made the experiment in a small way in connection with his own asylum. He met many difficulties, he tells us, through the "prejudices of the neighbourhood, and of the landlord of the cottage, who refused to continue to let it for such a purpose." Notice the significant admission that follows: "The patients, also, tired of the plain fare of the cottage family-table, and sighed for the flesh-pots of Egypt and the well-regulated comforts of the asylum. I therefore have for the present given up the experiment." He also remarks on the very small number of patients whom he found available for the trial. Out of two hundred and fifty female patients he found it difficult to pick out six who could be placed in the cottage. "I had, also," he adds, "frequently to change them, as, when the novelty wore off, the patients were many of them urgent to return to the asylum." The cost he found greater than in the asylum. In spite of his unsuccessful trial, Dr. Robertson's faith in his plan remains unshaken. He attaches importance to the family influence to be exerted upon his cottage patients by their custodians. Still, he proposes to assemble all the patients, for their mid-day meal, in one common dining-hall,—a proceeding not likely, one would imagine, to make them forget they were in durance as insane persons. Moreover, as he proposes putting only two or three patients in each cottage, to accommodate thus any considerable number, either the cottages must be so widely scattered as to make supervision almost impossible, and the coming to one dining-hall very inconvenient, or they must be so crowded together into a compact village as to sacrifice the advantages claimed, and even resemble Gheel, which he condemns.[1]

The Scottish Commissioners of Lunacy make the sapient suggestion that all the cottagers who are to receive insane boarders should receive full instructions as to the management of the insane! With this wisdom before his eyes, Dr. Clouston, in his Report of the Cumberland Asylum for 1870, still anticipates difficulty in finding among the small farmers and villagers fit custodians for the insane. Anxious, nevertheless, to propose some improvement upon the long-tried system, he suggests hiring relatives to take care of their own insane: a suggestion which shows into what absurdities a generally sensible man may be driven in the endeavour to accomplish the impossible. If any one fact relating to insanity may be regarded as immutably settled, that fact is the baneful results to all concerned of keeping a chronic lunatic in his own family. As if to redeem his character for good sense, Dr. Clouston makes the shrewd and truthful remark, that those hospital patients who complain most bitterly of their confinement, are usually the very ones utterly unfit for liberty and surest to abuse it.

Besides those patients whose cottage-homes are clustered into districts, with or without some connection with a neighbouring asylum, there are in Great Britain many thousands of single patients boarded out in private families, with no medical supervision. Of six thousand such in England, the Lunacy Commissioners report that their condition "is by no means satisfactory," whether with friends or with strangers.

The "American Cottage Plan"—to adopt a name used by some of its advocates—differs in its details and in its outward aspect nearly as much from the Gheel and Scottish as from the existing congregate plans. We hope to show, how-

[1] Journ. Men. Science, vol. xi., Ap. 1865.

over, that it may be reasonably expected to exhibit upon trial all the faults of the Scottish system—containing as it does all the elements of weakness, whose practical results we have just seen in the latter. For, like this, it aims at dispensing with locks; at allowing greater personal liberty; at cheaper accommodations; at the accomplishment of more work. Like this, it involves the placing of patients under the immediate control of work-people, and more or less remote from strict medical supervision.

This plan, whose prospective benefits to the States and to their insane wards have been so enthusiastically urged, both before the Illinois conference and in the annual reports of one or two superintendents of hospitals, consists of a central hospital building much like those now in use, surrounded by a dozen or two of smaller houses. Each of these last is to be under the charge of a man and wife, who will have under their care as boarders from twelve to forty lunatics, quiet and willing to work. Upon these buildings are to be no locks nor bars. Recent cases, and any others requiring close attention from the medical officers, or needing seclusion, or the restraints of locked doors and grated windows, are to be kept in the central or hospital building. To this also may be temporarily transferred those cottage inmates who may have transient paroxysms of excitement. From it to the cottages are to be removed those who lapse into dementia, or chronic mania of a quiet type; and perhaps also the convalescents who desire greater freedom with employment. According to one prominent advocate of the plan, the entire premises—which should be very extensive, to carry out the design and afford space for agricultural operations on a large scale—are to be surrounded by a high wall. The whole establishment, hospital and detached cottages, is to be under the rule and government of a medical superintendent.

For this system, as compared to that of congregate hospitals, it is claimed, that it is cheaper in first cost of buildings and in maintenance; promotive of greater content and happiness to the patients; less repulsive to the feelings of both patients and their friends; less subversive of that liberty which is the natural right of man; and more conducive to recovery.

We believe this proposed new system to be impracticable, or if practicable, in every aspect inferior to the existing form of hospitals. What appear as real advantages to be gained, can be equally well obtained in congregate hospitals, provided only these can obtain the liberal grants of land, and the model men, women, and labourers, which seem to be assumed by the advocates of the cottages.

Before stating in detail our objections to the new plan, and its inferiority in various points to the ordinary hospital, we willingly admit that the insane asylums of to-day are by no means faultless. Some of the complaints made against them are entirely unreasonable, springing from the nature of the disease treated rather than from any fault of the system. Others, however, have some foundation, but are due not to radical errors in the plan, but to its improper execution. For instance, we must admit that many of our State asylums for the insane are much too large for the comfort and well-being of the inmates. Their halls and dormitories, too, are often injuriously crowded. Where one man must look after from four hundred to seven hundred patients, he can hardly give to individuals the personal attention their cases may require. The management of so large a household necessitates the enforcing of a sort of rigid, cast-iron, routine discipline, which cannot yield to the needs of exceptional cases. The particular wants of the one curable patient are apt to be lost sight of in attending to the general comfort of his twenty incurable comrades. When, too, the superintendent bears the burden of financial and agricultural management, he is still less able to give the proper individual care to his patients.

Another acknowledged fault of many of our asylums is the deficiency of land and other facilities for employing patients.

The mere statement of these defects shows them to be, not essential but accidental. More, and smaller, hospitals, with plenty of land, are the proper remedy. Other alleged evils found in the existing hospitals will be better referred to when we consider the beauties and felicities claimed for the new plan.

Let us now assume the existence of an establishment consisting of central hospital and one or two dozen cottages, each of the latter to contain forty patients. The difficulty which first occurs is the finding of married couples, fitted and disposed to keep a boarding-house for forty lunatics, and to superintend their labour, their clothing, their general conduct and manners, their getting up and lying down, and their daily food. Remember, in this connection, that with all their care and responsibility, our married friends are not the masters of their own household, but subject to inspection and criticism from certain officers who may intrude upon them at any hour of day or night. If it be said that high wages will secure the right persons, we reply that high wages cannot be given, for the new plan is pre-eminently to be a saving plan.

If it be difficult to secure good attendants for existing hospitals, even when man and wife are employed together—and it notoriously is so—it would surely not be easier to find persons fit to manage a large family of insane people, in a way to combine the greatest amount of happiness with the greatest amount of work. Why putting certain persons, with a tolerably well-known average of good and bad traits, in command of a house and forty patients should develop any especial fitness to soothe and control the insane mind, not potentially existent before in the same persons inside of a hospital ward and under the watchful eye of an educated, philanthropic, and responsible physician, we are utterly unable to see.

Supposing, however, that we have our cottages duly officered, and filled, each with its two scores; the man and wife possessed of the qualities fitting them for superintending at once, housekeeping, farm, or other labour, and for intelligently observing the changes occurring in their inmates, how would the well-known traits of the insane be likely to manifest themselves in this cottage life?

Even among the quiet class thus placed, few groups of forty would fail to include one or more patients whose nocturnal restlessness would prompt them to stroll forth through the unfastened doors. How many of them would conscientiously return in time for breakfast, carefully opening and shutting door or window with tender care for the health and the quiet slumbers of their comrades; how many would get into trouble; how many might enter rooms where they were not wanted—even into the cottages inhabited by the other sex—it is unpleasant even to speculate upon. It cannot be expected that the attendants who have worked all day, with and for their boarders, should keep awake all night to watch them; and a night-watchman for each cottage would be a considerable additional expense.

Besides purposeless straying, elopements must be frequent. It is absurd to say that the patients occupying the cottages would be exclusively those indisposed to abscond, and that the amount of freedom there enjoyed would remove all wish to escape. In countries inhabited by a simple, stolid, and ignorant peasantry, with no ambition or aspiration in life beyond food, drink, and warmth, accustomed to pass their days where fate placed them, with no dream of ever changing their position in the social scale, and never the faintest idea of disobeying the powers above them—amid such a population this plan might succeed tolerably well so far as custody is concerned. But fancy a wide-awake native American, a free, independent citizen, calling no man master, full of plans, and

schemes, and hopes, restless, accustomed to journey thousands of miles for business or pleasure,—fancy such a man contented with the liberty of the so-called cottage, with its humdrum routine of labour, the same yesterday, to-day, and forever! When sunken in the deepest depths of dementia, and only then, could such an one be held for months and years without locks and bars. Or consider even an Irish pauper, who has been in this country long enough to vote and to imbibe the intoxicating sense of his sovereignty; is he the man to pass busy days and quiet nights under the mastership of one whom he thinks no better than himself, and to work moreover without wages?

Of course we do not deny that in every collection of insane people there are a few who may be safely trusted to go and come without eloping. But we do utterly deny that two-thirds of the chronic, quiet insane of a State, can be held in these open houses, without frequent elopements. If these do not occur, it will be due to the "substantial wall, inclosing the premises" which forms a part, not very consistently, of the plan of some of our alienist "freedom-shriekers." To speak seriously, this wall, while it could not be built high enough to retain an earnest seeker after liberty, would doubtless be amply adequate to the dissipation of any sentimental notions of freedom which might have arisen in the mind of some delighted lunatic upon viewing the barless window and the lockless door. Possibly, however, the docile inmate might be brought to regard it in the pleasanter aspect of a barrier against the intrusions of outside barbarians.

We believe, too, that the sane have some rights as well as the insane. One of these rights is, to be protected against undesired insane visitors. A patient who is generally harmless may have homicidal propensities regarding some of his relatives. Or one usually tranquil may have periods of dangerous fury, during or just preceding which, he would be especially liable to elope and particularly dangerous to be at large. Even some patients considered perfectly harmless, may yet, by their strange ways, terrify women and children. Against all this danger and annoyance, society has a right to protection. It has a right to demand such custody of the insane as will insure at once their welfare and that of their sane neighbours. The patient himself has also a claim to be protected against the possible results of elopement. Drowning, freezing, starving, railroad slaughter, these are results that have occurred and will occur to maniacs and demented people allowed their liberty or entrusted to just such mockery of safe-keeping as must necessarily exist in these cottages.

This consideration, to which we have adverted, of the great difference between the temper and habits of our people and those of middle and lower class Europeans, is of much importance, and has relations with nearly every aspect of the "cottage plan." Not only does it bear upon the degree of contentment and the number of elopements to be expected, but it seriously affects the industrial aspects of the plan. "No," the American or Irish-American would say, "I will not work in a gang for the State, and without pay."

The increased difficulty, if not impossibility, of adequate supervision by the medical officers, is a most serious objection to the new plan. The approach of an officer to a cottage can be seen and prepared for. All superintendents know how close must be the watchfulness, how frequent and unexpected the visits, even to the compactly clustered wards of a congregate hospital, to prevent irregularities, laxities, negligence, and even abuse. Who believes that proper discipline can be maintained when there are a score of detached buildings scattered at considerable distances? It is idle to say that those having charge of the cottages must be persons of high character and intelligence, so as not to require watching. Practically and in the long run the excellence of the keepers

will be proportioned to their payment. And as we have said in another connection, such salaries will not be paid as will secure men and women of character and culture, above, or even equal to, that of the small farmers, mechanics and tradesmen in the surrounding district. Hence there will be just as much need of close supervision in the new as in the old system. As to the facilities for exercising this vigilance there is no comparison between the two. A hospital ward can be entered by the officer, unseen, from either end, from above through the attic, or from below through the cellar, in or out of a regular order, and without the possibility of warning being given. How different with outlying cottages! If we complain, and justly, that one superintendent with two or three assistants cannot adequately care for five or six hundred patients under one roof, what can be expected when these or twice the number are dispersed in detached buildings, accessible only through the open air? What shall be thought of the plan deliberately put forward by the trustees of the Willard Asylum for the Insane in New York, in which is one building, to contain two hundred female patients, situated *one mile* from the main hospital? How is the superintendent of the fifteen hundred patients, here to be domiciled in a dozen different buildings, to be able to meet his responsibility when part of his charge is a mile away from his office? If it be said that some local officer will be responsible for the good management of the remotest building, then we reply that this one ought to be independent and supreme—not trammelled and harassed by a nominal superior who cannot have intimate personal knowledge of things so far removed. Nothing so surely leads to abuses as a divided and unsettled responsibility. It seems to us that no man with any approach to an adequate sense of the obligations attached to the position would consent to be regarded as the supreme head of an establishment whose members are scattered at such magnificent distances.

In a discussion by the members of the Association of Superintendents of Insane Hospitals, at the meeting held in Toronto in 1871, upon a paper presented by Dr. Edward Jarvis, advocating the new system, nearly every speaker expressed distrust and disapproval. Dr. Thos. S. Kirkbride gave his experience of the evils arising from defective supervision, in a trial of the cottage system on a very small scale made by him many years ago. From entertaining great expectations of benefits, he reluctantly passed to entire disapproval and abandonment. And in this instance, we should suppose, the idea was tested under conditions the most favourable possible.

As one out of many possibilities of evil connected with this boasted progressive movement, we do not see how any superintendent could feel that his female patients were entirely secure from injuries of a sort too lamentable and repulsive even to be named. Between inadequate supervision and undue liberty, a door is opened for innumerable abuses.

Some friends of the new system lay especial stress on its greater cheapness. They declaim upon the enormous cost of the mammoth asylums of the day. Forgetting that, from a liberty-loving point of view, they have just been denouncing them as gloomy "prisons," with "cells" and "dungeons," they now inveigh against the extravagant policy of placing paupers in "palaces!" We cannot at all understand how they expect to shelter, warm, feed, clothe, watch, employ, treat, and care for a given number, under twenty roofs more cheaply than under one. Nor have we seen any explanation, except such as seemed to point to inadequate provision and penny-wise economy as the only secret. Every one knows that it is the proportionate cheapness of a large as compared to a small hospital, that has led to what we have pointed out as the excessive and unwieldy magnitude of many State institutions. In answer to all allega-

tions of extravagance in providing for the insane, we maintain that we could not possibly have anything plainer, simpler, or cheaper, consistently with the objects in view, and with a true economy, than are our existing hospitals. The talk about the inexpensive character of the edifices contemplated by the new plan, is a mere tissue of glittering generalities. "The buildings," say our visionary friends, "should be plain, substantial, and inexpensive." Of course they should; and the same aim has been kept in mind in the construction of congregate asylums. The dictum is unobjectionable; as perfectly harmless and inane as Dr. Bemis's opinion that, in the vicinity of every new-plan asylum, "there should be found a few well-to-do families, who would be willing to receive one or two lunatics, such families being governed, so far as their lunatic boarders are concerned, by all the rules and regulations of the hospital!"

We are equally unable to perceive, how, once started, the cottage system can be maintained more cheaply than the congregate. Unless we assume that its advocates contemplate diminishing the comforts to which our insane have been accustomed, we must suppose that the economy of maintenance which they claim rests upon the assumed increase of productive industry. But why should the same patients, living in a so-called "cottage," work better than in a "ward?" There is surely nothing to prevent the providing of all encouragements to industry in the one case as well as in the other. The practical trouble in both is to induce the patient to use the means given. One gentleman thinks the reason patients in congregate asylums accomplish so little work, is because the persons who lead and direct their labours are "mere nurses;" but is this necessary? Another finds the cause in the discouraging necessity for the would-be labourer to take off his slippers and put on his boots, every time he goes out to work! We have not noticed it to be a part of the new system that patients should sleep in their boots. Yet, somehow, they are to work with an alacrity and efficiency heretofore unknown. We are disposed to believe that the advocates of the cottage-system, founded as it is on foreign models, have unconsciously confounded the traits of American patients with those of the European and Scottish pauper lunatics, and are thus anticipating labour-results much more important than they can ever attain. The question whether our chronic insane cannot be made more productive, is entirely independent of the relative merits of segregate and congregate hospitals.

Much stress is laid, by some reformers, upon the greater amount of personal liberty to be enjoyed under the new system. Now we have shown how grave may be the evils attendant on this boasted freedom. We have given reasons why the vast mass of insane patients should not be left to go and come at their pleasure. But if any physician, having charge of the insane, believes that a certain class would be benefited by liberty, why need cottages be built to test the idea? It surely is perfectly practicable to assemble such patients in certain wards of the ordinary hospital, from which the locks and bars shall have been removed. Indeed we once saw this experiment tried, and with very unsatisfactory results. Practically, in every hospital, the very few patients who are sure not to abuse it, can be given a great deal of liberty. If the whole truth were told, indeed, we believe that in most of our large State asylums, very much more mischief arises from too great, than from too little, freedom. We would express, too, our hearty concurrence with Dr. Clouston—an advocate of the cottage-system—in his belief that those who clamour loudest for liberty are surest to abuse it. The vast majority of the patients who affect to be shocked and wounded by the sight and sound of bars and locks are of that difficult, captious, and exacting class who are never contented in or out of hospitals, never grateful for favours, but who consistently complain of everything and

everybody, and uniformly abuse any privilege that may be granted. As this class make much noise in the world, and write to the papers, and employ lawyers, and delude clergymen, their complaints are often mistaken for those of the insane in general.

It is hard to understand how gentlemen conversant with the insane in this country can believe that they are capable of adequate and agreeable control in these open cottages. Do they not every day see patients submitting as a matter of course, and without a murmur, to the organized discipline and restraint of a hospital, complying cheerfully with the requests of attendants whom they see and know to be acting under the immediate direction of the chief medical officer? And do they not also know that if, upon these same patients, any such restraint or coercion were attempted in the family or in the boarding-house, it would be instantly and angrily resisted? Do they not likewise know that just the degree and kind of restraint exerted by the rules and customs of a hospital are often directly and powerfully soothing and curative in their influence?

Much delight is expressed by the friends of the proposed improvements at the idea of a "family life" to be enjoyed by the cottage patients. The pictures of life in a cottage, as given us from Gheel and from Scotland, are surely not altogether charming. A writer to whom we have not before referred, upon "Gheel in the North,"[1] describes the sort of family comfort enjoyed by insane boarders in a part of Scotland considered the most enlightened. The most cheerless attic, the gloomiest cellar, the worst bed, and the poorest scraps of food, make up the home comforts of these unfortunates. But in our proposed American cottage-system, what sort of family life is that which associates together a sane man and wife, with or without children, a female servant, and forty, or even a dozen, chronic lunatics? Even among sane people, a large boarding-house for men has few home-like charms. If in some cottages a superior class be placed, as of educated convalescents, they would not especially delight in associating with an illiterate couple and their Irish "help." Nor would they easily submit to control from such. Some, however, would strictly limit the system to caring for the chronic, pauper insane. Now these —former denizens perhaps of poor-houses, or sunk in dementia—would not appreciate any of the advantages claimed for the cottage life. We acknowledge, however, that those who would thus limit the use of the cottages, do not claim for them much beyond economy.

Admitting a real advantage derivable by insane men from the society of good and kind women, we fail to see why it cannot be as well obtained, and with less fear of abuse, by the practice of placing a man and wife in charge of each male hospital ward. This is done, with alleged good results, in some of our large asylums.

Greater facilities for classification are claimed for the "American Cottage Plan." This claim, like some others, seems utterly groundless. Surely, wards holding from ten to twenty-five patients afford every convenience for this purpose that could be found in detached houses, holding probably twice or thrice the number.

Some persons who oppose the old plan for providing for the insane, and favour the one we are considering, tell us that, in the congregate hospital, patients encourage and strengthen each other's delusions; also, that quiet patients are seriously injured by loss of sleep due to the ravings of turbulent comrades. Hence they argue that patients should rather be separated than

[1] Journ. of Men. Science, July, 1865.

massed together; and especially that noisy ones should be remote from those who are quiet. The first assumption is contradicted by practical acquaintance with the insane. So far as they compare or discuss their delusions at all, each is usually able fully to appreciate the absurdity of his neighbour. Far from supporting each other's false beliefs they are much more apt to ridicule and oppose them. As to the second charge, we would remark that there are very few hospitals which cannot give perfectly quiet lodgings to patients who are in a condition to be really disturbed by the noise of maniacal companions. Many of the complaints upon this score, too, come from the sort of people we have before described as seeking always for something to find fault with. The accounts of disturbance given us by this class are usually grossly exaggerated. We do not believe that in a well-managed and not overcrowded hospital any patient suffers from this cause habitually, or to an extent affecting health, or preventing recovery. Perhaps the evil would not be much less if, in place of disturbing his fellows, the maniac should keep a sane family awake.

Sad as is the fact, we all are aware that with all the machinery of restraint, watchfulness, supervision, and careful medical treatment found in the best-managed hospitals, the most melancholy casualties will now and then occur. Suicide, homicide, and catastrophes by fire or water, will happen. When we remember how subtile are often the movements of the insane mind, how hidden are sometimes the motives that lead to terrible deeds, and again, how utterly careless of danger most demented patients are—ought we not long to hesitate before substituting for the insane hospital of to-day, the loose and ill-contrived schemes we have been considering?

Perhaps we have not sufficiently noticed a certain confusion of purpose and motive among the friends of the new method. Some regard the matter purely from a philanthropic or sentimental point of view. In their minds prevail vague but seductive notions of "liberty to the captive," fascinating pictures of a life of Arcadian simplicity, with the lowing of herds, bleating of flocks, sweet-smelling flowers, and delicious moonshine! We have seen how different is the reality, as shown at Gheel and in Scotland. Certainly the American plan, with its forty boarders in one "cottage," would not seem to promise a much more complete realization of these pleasing dreams. But were it to prove never so Paradisiac, it would not suit the American citizen. Unless demented to the lowest degree, he prefers to choose his own residence, companions, and pursuits. This preference he will be very apt to carry into action if he find the doors open. The other branch of the reforming party look upon the matter only in its economical aspect. We think we have shown that no important saving can be made by the proposed change, except at the cost of the unfortunate class most concerned. If this plan be carried out, and saving be effected in the cost of maintaining the insane, let no one be deceived as to the character of the movement. It will be no progress, but a retrogression—a falling back toward the old poor-house management, under which has existed such unspeakable wretchedness.

It is perhaps not very wonderful that the general public, influenced by the confident promise of economy, and by the idea of greater liberty to be enjoyed by the patient, should look with favour upon the proposed system. But how any physician who has been for years familiar with the insane can regard the new plan as an improvement, we cannot understand. B. L. R.

ART. XIX.—*Transactions of the Clinical Society of London.* Vol. IV. 8vo. pp. xlii., 200. London: Longmans, Green & Co., 1871.

THE fourth annual volume issued by the Clinical Society begins with the annual address of the President of the Society, Sir WILLIAM GULL. It contains some valuable suggestions as to the proper method of prosecuting clinical study, but, unlike many other able physicians, Sir William does not believe that hospitals furnish the only, or even the best, field for clinical study. The opportunities of private practice, he says, if carefully utilized, might solve for us many obscure problems. The accurate study of the cases of infectious diseases, which can only be observed in private practice, sometimes will teach us through what ways the infection invades the organism, and will enable us, if not to obviate its progress, at least to learn something more of the means for controlling it.

"The Society," he says—and what follows is of equal applicability to medical societies in this country—"has two functions to fulfil: to exhibit the working of the most critical methods of research—to show, in fact, what clinical medicine should be, and to improve these methods. For myself, I am far from believing that he is the best observer who records the greatest number of facts, but he who has the perception which enables him to separate the chaff from the wheat—what is essential from what is accidental."

We shall first invite the attention of our readers to the medical papers contained in the volume.

Art. IV. *Two Cases of Acute Scurvy.* By HENRY G. SUTTON, M.B.—In both the cases reported by Mr. Sutton the affection set in like an acute disease. One patient felt so cold that she had to sit near the fire, and the other had a rigour. The temperature was never below 102° F., and it reached 104° F. in both cases; the pulse ranged from 120 to 140. The duration of the symptoms was in one case twenty-nine days, in the other only eighteen days. In the first case there was an almost entire absence of the usual ecchymotic appearance of the surface of the body; but, on the other hand, there were numerous ulcers and circumscribed spots of extravasated blood in the mucous membrane of the mouth. And after death similar ulcers and spots were found in the mucous membrane of the stomach and colon, and the latter also were seen on the pleura and pericardium. The tonsils when first seen were covered with a gray substance which was mistaken by a surgeon who saw it for the results of syphilitic inflammation, and by a physician for those of diphtheritis, but was simply a slough of the mucous membrane, and did not re-form when once removed. In the second case the lesions of the mucous membrane were not so extensive, but the characteristic extravasations into the skin were present. In both cases vaginal hemorrhage was the symptom which ushered in the disease.

Art. XIV. *On Circumstances influencing the Safety of Subcutaneous Injection of Morphia.* By C. HANDFIELD JONES, M.B.—The cases which Mr. Jones reports show that in certain individuals the subcutaneous injection of morphia gives rise to alarming symptoms, and he is therefore inclined to agree with Dr. Harley, who says that morphia should never be injected alone unless there is reason to know that the patient will experience no ill effects from it. In all other cases it is better to combine it with atropia. Dr. Anstie also thinks it unnecessary and unsafe to commence with larger doses of morphia than one-sixth grain, and in slight cases he finds one-sixteenth of a grain sufficient. Mr.

Jones says "it might be well if the Clinical Society brought their experience to bear upon the matter, especially with the view to discover whether there are any objective signs of the state in which opium is not tolerated, and whether, as the last case I mention suggests, such a state may exist at one time and not at another."

Art. XV. *Paroxysmal Hæmaturia.* By FREDERICK W. PAVY, M.D.—This paper contains a tolerably full report of one case of intermittent hæmaturia and a brief reference to another. The patient in the former case was a man fifty-two years of age, who, as the result of a chill brought on by long-continued exposure in an open dog-cart to cold, passed urine of a dark porter-like colour. Eleven days later, having resumed his ordinary occupation, and again, after a similar exposure, he experienced a repetition of his former attack; others subsequently occurred, after both driving and travelling by railway. The patient was himself disposed to attribute the attacks to the effects of the jolting movements of riding. He had noticed, however, each time before passing the dark-coloured water, that the attack was ushered in with a chilliness, shuddering or shivering, and nausea. Since the first attack there had been no vomiting. It was not long before the patient clearly ascertained that the attacks owed their origin to cold and not to motion of the body, and was able to protect himself against them by means of clothing. When the urine was examined microscopically, no blood corpuscles could be seen, whilst there were visible casts of tubules, dark granular particles and crystals of oxalate of lime. The urine was highly albuminous.

Art. XVI. *On Veratrum Viride in Rheumatism.* By ALEXANDER SILVER, M.D.—Dr. Silver, having become convinced that veratria was a constituent of Laville's Gout Liquid, which is possessed of some efficacy in the treatment of gout and certain forms of rheumatism, resolved to try an officinal preparation containing veratria in any rheumatism cases which he might be called upon to treat. Tincture of veratrum viride was the preparation selected, and was administered in doses of two minims every two hours during the day, the dose to be doubled and given every three hours during the night. The medicine was regularly exhibited until sickness was induced, and this was usually within forty-eight hours of its first administration. It was then intermitted for a time, to be again resumed, if necessary, when the nausea had passed away. He claims for veratrum viride the power to relieve pain and reduce the temperature. Under its use the urine became more copious and clear, urates ceasing to be deposited. The remedy has also, he thinks, as much power in preventing the occurrence of cardiac complications as any other usually employed in the treatment of rheumatism. The cases which Dr. Silver reports were only of moderate severity, and it does not seem to us that the duration of the disease was shortened in any one of them by the use of veratrum viride. Some years ago we had the opportunity of observing some cases of acute rheumatism treated by the tincture of this drug, and recollect that, further than a diminution of the frequency of the pulse, it seemed to be without influence upon the disease.

Inasmuch as it has been recently shown that veratrum viride does not contain veratria, it is rather strange that Dr. Silver should ascribe whatever therapeutic power it has to this alkaloid.

Art. XVIII. *A Case of Anasarca treated by Puncture.* By C. HANDFIELD JONES, M.B.—In this communication Mr. Jones reports a case of anasarca dependent upon disease of the kidneys. In order to relieve the great swelling of the lower limbs, the following plan of treatment was adopted: The patient, being well surrounded with blankets, was placed in an arm-chair by the fire, and a

trocar of rather small size was introduced into each calf and, the point being directed upwards and the stylet withdrawn, allowed to remain for several hours. Sixty measured ounces of fluid escaped from the right leg, but only ten from the left, in consequence, it was thought, of the canula having penetrated too far, and not lying properly in the subcutaneous areolar tissue. The fluid drawn off was pale, weakly alkaline, faintly yellow, having a specific gravity 1014, highly albuminous, and contained floating films of fibrin. The fibrin was quite devoid of corpuscles, and had a fibro-granular structure. The punctures made by the trocar were only first apparent the day after they were made. The operation was repeated three days afterwards, when 120 ounces of fluid were collected, besides a great deal which ran out of the punctures for several days after, enough to saturate three blankets. The treatment afforded much relief, but the patient died soon after the operations of erysipelas of the face. Mr. Jones thinks that the absence of corpuscles from the fluid, the escape of fibrin from the vessels without the existence of inflammation, and the lower specific gravity of the fluid as compared with that of the serum of the blood are points of interest in the case. Less injury is done to the skin, and less irritation produced by one large puncture than by many small ones, he thinks, and the details above given show that the quantity of fluid evacuated in a short time may be very large.

Art. XIX. *Two Cases of Chorea, with Urinary Analyses.* By C. HANDFIELD JONES, M.B.—In the two cases of chorea which form the basis of this paper, the results of the chemical analysis of the urine appear at first sight rather contradictory, but they cease to seem so when we take into consideration that the first case had been suffering for eight weeks before she came under observation, while the second had only been ill about a week. The following tabular statement enables us to compare the two cases:

1st Case, Female, Age 11 *years.*

	Acme.	Decline.	Weight.
Urea . . .	111 grains	345 grains	50 lbs. in acme.
Phosphoric acid	9.67 "	35.5 "	63 " " decline.
Uric acid . .	0 "	5.2 "	

2d Case, Female, Age 13 *years.*

Urea . . .	517 grains	268 grains	51 lbs. in acme.
Phosphoric acid	60.5 "	14.5 "	68 " " decline.
Uric acid . .	5.4 "		

It is quite possible, Dr. Jones thinks, that in the first case the period of excessive urinary elimination had passed by, and that, had the analysis been made earlier, the results would have been similar in both cases.

Art. XX. *Cases of Paralysis of the Soft Palate, &c., resembling Diphtheritic Paralysis.* By W. H. BROADBENT, M.D.—Dr. Broadbent has had the opportunity of observing a case of paralysis of the soft palate, and even of paralysis of the larynx, presenting precisely similar symptoms as are presented by cases of paralysis following diphtheria. Since no history of diphtheria could be obtained in either instance, he thinks it may be concluded that the so-called diphtheritic paralysis is not a specific disease, but a form of nervous paresis resulting most commonly from the effects of diphtheria on the system, but capable of being produced by other acute diseases, and by debilitating causes generally. He is also inclined to look upon this form of paralysis "as an affection of the nerve trunks, by which their conducting power is, for the time being, impaired, and as intermediate in respect of locality between the ordinary forms of hemiplegia

and paraplegia, due to lesions of the brain and cord, and infantile paralysis," which, with Mr. Barwell, he considers to be an affection of the peripheral termination or end-organs of the motor nerves.

Art. XXII. *Case of Paralysis of the Ophthalmic and Superior Maxillary Divisions of the Fifth Nerve, of the Fourth Nerve, and of the Branch of the Third to the Levator Pelpebræ on the Right Side, from Syphilitic Disease of the Base of the Cranium.* By W. H. BROADBENT, M.D.—The most interesting feature of this case is the paralysis of the fourth nerve, which, however, was recognized some time after the patient came under Dr. Broadbent's care, in consequence of the double vision, which could not be explained by the trifling amount of strabismus present. "It was found," he says, "on more careful examination, that the two images were obliquely placed with respect to each other; one window, for example, he said was before him, the other on the floor to the right, and a curious fact, which I have not observed in paralysis of the third or sixth nerve, or seen noted, was that, when he closed the left eye, and tried to walk with the aid of the right only, he staggered about as if drunk. This oblique position of the pseud-image below and to the right of the real image, showed that the superior oblique muscle, and therefore the fourth nerve, was the one paralyzed, and the third nerve, with the exception of its branch to the levator palpebræ, was apparently not involved."

Art. XXIII. *Cases of Tinea Circinata (Ringworm) communicated from the Horse.* By TILBURY FOX, M.D. Communicated by Dr. BUZZARD.—Dr. FOX has had the opportunity of observing tinea circinata in seven men, all of whom had taken it from a pony. The cases were all marked; the inflammatory aspect was severe; the infiltration decided; the extent of the eruption greater; and the herpetic character, when the earlier stages were observed, not at all abortive, but much more distinct than usual. In one case, the fungus, luxuriating amid the textures of the skin, set up so much irritation as to induce pustulation in place of the ordinary herpetic vesiculation in certain of the patches. This exaggeration of the features of the tinea circinata was attributed to the plentiful implantation of the fungus germs in unusual abundance and luxuriance. The disease, in fact, looked like an eczema, and might have been readily taken for it—a mistake which Dr. Fox thinks has been made in other cases. He paid a visit to the pony, and thus describes the appearances which it presented:—

"I found its body covered all over with discoloured spots of a fairly circular outline, varying in size from that of a shilling to large irregular patches the size of the palm of the hand and more, the latter formed, I imagine, by the rapid spread of the disease, over a large track. The appearance presented was as though circular pieces of mud had been scattered all over the white coat of the pony, and the mud had been brushed off, leaving the hair over the circular areas with which it had come in contact dirty-white, contrasting with the clear white of the intermediate portion of the coat. But on looking closely to the discoloured patches, it was at once evident that the hairs themselves had become altered in texture and direction. They were curled and bent out of their proper, and into wholly different, directions. They were loosened, and readily came away from the follicle; some broke off; they were also more opaque, and the surface of the skin was covered over by a mealy powder, thickly set about the hairs."

Art. XXV. *Cases of Roseola Variolosa.* By ALFRED B. DUFFIN, M.D.—This paper was noticed in the July number of this Journal for 1871, page 257.

Art. XXVIII. *Cases of Skin Disease treated by Phosphorus.* By W. H. BROADBENT.—An abstract of this paper will be found in the July number of this Journal for 1871, page 253.

Art. XXIX. *Case of Left Hemiplegia, with Total Loss of the Right Eye.* By FREDERICK SIMMS, M.B.—Dr. Simms reports the case of a woman who, an hour after severe flooding following labour, became unconscious, and on recovery suffered from severe aching pains in the left arm and leg, both of which by degrees became paralyzed. This paralysis was followed by a gradually increasing rigidity of the flexor muscles of the affected limbs, by startings and involuntary movements of flexion and extension, all which ended in permanent rigidity of the flexors of the forearm, so that the hand became bent upon the limb, and could not be kept extended. Three days after her delivery, inflammation of the whole eye came on, ending in blindness; speech never being much impaired. Slight improvement followed the use of the iodide and bromide of potassium, and of iron, with liberal diet. From a review of the symptoms, Dr. Simms is inclined to believe that they were due to embolism of the internal carotid, at or near the point where it gives off the ophthalmic branch.

Art. XXX. *Cases treated with Digitalis as a Topical Remedy.* By F. ROYSTON FAIRBANK, M.D.—Dr. Fairbank, having observed that digitalis, when administered internally for the purpose of allaying febrile symptoms, has the power to relieve inflammations and congestions, instituted a series of experiments with the view of determining whether it would have the same effect when applied directly to the inflamed or œdematous part. The results were exceedingly favourable; in fact, he says, "I have never found any other application act so rapidly or so certainly." The treatment is not altogether new, having been used by M. Debout in hydrocele, and by Dr. Besnier in cases of orchitis. The mode of application of digitalis topically which he adopts, is to make a decoction of the dried leaves, in the proportion of a small teaspoonful to half a pint of boiling water, or a drachm of the tincture may be used instead of the leaves. Flannels should be wrung out in the decoction, and applied in the usual way. Another way of applying the drug is to foment the part affected with simple hot water, and then to rub in a little of the tincture.

Art. XXXII. *Case of Diphtherial Paralysis treated with Faradism.* By EDWARD HEADLAM GREENHOW, M.D.—This case presented to a very marked degree the ordinary symptoms of diphtherial paralysis. Dr. Greenhow being desirous of trying the effects of faradism uncomplicated by the influence of any other treatment, prescribed as medicine a draught of infusion of gentian, and faradized the patient at first daily, and at a later period on alternate days. The upper limbs were the first to regain their power. Twenty-one days after the treatment was begun, she was able to walk a few paces without assistance. From this time she steadily improved, the muscles of her limbs became plumper and firmer, and the improvement steadily progressed, until a complete cure was effected.

Dr. Greenhow calls attention, especially in this case, to the tenderness over the sciatic nerve, and as he has observed this in other patients, he thinks it suggestive, at least, of the occasional existence of neuritis—"a suggestion," he says, "which obtains support from the fact that my colleague, Mr. Hulke, has, in one case, verified the existence of neuritis in the optic nerves of a patient suffering from impaired vision consequent upon a recent attack of diphtheria.

Art. XXXIII. *Case of Diabetes, treated with Opium.* By HENRY THOMPSON, M.D.—Dr. Pavy has recorded, in a previous volume of the *Transactions*, a case of diabetes successfully treated with opium. In the case reported by Dr. Thompson, no improvement followed the use of opium; on the contrary,

symptoms of drowsiness coming on, it was abandoned. The quantity of urine diminished, however, when tonics, alkalies, and a restricted diet were prescribed.

Art. XXXV. *Case of Cervico-Brachial Neuralgia treated with the Constant-Current.* By THOMAS BUZZARD, M.D.

Art. XXXVI. *Cervico-Brachial Neuralgia apparently cured by the Constant-Current. Double Occipital Neuralgia not relieved by Electricity.* By FRANCIS E. ANSTIE.—We shall notice these two communications together, because Dr. Anstie's communication seems to have been suggested by Dr. Buzzard's. Dr. Buzzard's patient was a woman, sixty-five years of age, who, after presenting some vague symptoms of cerebral disease, began to suffer from neuralgia of the cervical and brachial plexus of the right side. On sixteen occasions the constant-current was applied between the cervical spines and the shoulders or hand. The result was great relief on ten occasions, moderate relief on two, and very slight relief on four occasions. The process described as galvanization of the sympathetic, with the negative pole in the stylo-mastoid fossa, was followed on the one occasion in which it was used by great relief; with the positive pole in the stylo-mastoid fossa there was no abatement of pain. Faradism, on the contrary, undoubtedly did her harm. Dr. Buzzard says that the impression produced on his mind by the results of the use of galvanism, in this case, was a strongly favourable one. None of the other remedies which were used appeared to have any decided influence upon the progress of the case.

Dr. Anstie's case of cervico-brachial neuralgia appears to have been cured by the constant-current. The patient, a woman, although 48 years of age, presented no signs of physiological senility, *i. e.*, no grayness of hair, no arcus senilis, and no detectable rigidity of arteries. The general muscular system was also in a fair state of development. The treatment in the case of double occipital neuralgia was unsuccessful, not even the palliative effects, so often seen, were produced by the application of the constant-current; an opposite effect being produced by it on one occasion. All the other usual methods of treatment, including the hypodermic injection of morphia and atropia, were also found incapable of doing any good. In regard to the kind of cases of neuralgia in which the constant-current is likely to be of service, Dr. Anstie says, "As a general rule, it may be said that there is the best chance of its succeeding in cases where the patient has not entered upon the stage of tissue-degeneration, which marks the latter periods of life. But now and then, from some reason, which we are as yet altogether unable to explain, the current fails entirely, even in the case of a person (like the subject of my second case) who is as yet comparatively young."

Art. XXXVIII. *Accumulation of Hair, String, &c., in the Human Stomach.* By WILLIAM GULL, M.D.—Dr. Gull refers to the case reported by Dr. Best in the *British Medical Journal* for December, 1869, of a woman, in whom, during life, a hard nodular tumour was to be felt at the epigastrium, which, after death, was found to be caused by a concretion of human hair, weighing 30 oz. avoirdupois. The history of a very similar case came under the care of Dr. Godfrey, in which the patient died of peritonitis after a premature labour, and was communicated by Dr. Godfrey to Dr. Gull. There was a perforation, in the duodenum, which opened into the cavity of the abdomen. In the stomach was found a large mass of hair with strings, &c., which extended through the pylorus into the duodenum and through the small intestine. The concretion, weighing 5¾ oz., consisted of string, thread, cotton, wool, and hair of three colours—the hair of her own head and that of her children. A third case is briefly

referred to in a letter from Dr. Thorowgood, in which the mass of hair weighed 26 ounces.

The habit of swallowing hair is not peculiar to the human subject, for it is very common among the Angora breed of cats.

Art. XXXIX. *Case of Hemiplegia in a Syphilitic Subject.* By J. HUGHLINGS JACKSON, M.D.—Dr. Jackson says there are at least three different ways in which syphilis leads to hemiplegia.

1. Syphiloma of the surface of the brain leads to an epileptic seizure, which is followed by hemiplegia, the epileptic hemiplegia of Dr. Todd.

2. A nodule grows in the motor tract itself, and if it produce hemiplegia at all, the palsy comes on very slowly.

3. The middle cerebral artery, or some branch of it, is the subject of a syphilitic growth, its channel becomes plugged, and as a consequence, there is hemiplegia from softening of the motor tract, and there may be loss of speech from softening of convolutions.

The case which he reports was produced, he thinks, in the third way. It was one of right-sided hemiplegia and nearly complete loss of speech came on gradually without much loss of consciousness. It is to be recollected, in the management of these cases, that the softening of the brain, which is the indirect consequence of syphilis, is to be treated, and upon this the iodide of potassium has very little influence.

Art. XLI. *Facial Neuralgia, re-induced in a Person who had previously suffered from it, by Syphilis, and attended with Ocular Paralysis, and various other Lesions.* By FRANCIS E. ANSTIE, M.D.—The points of interest in the case reported by Dr. Anstie are, first that the neuralgia, although undoubtedly connected with syphilis, and vanishing under treatment by iodide of potassium, was strictly limited to the same side which had been previously affected with hemicrania, and that the paralyses of common and special sensation, except that of smell, and of the ocular muscles, were similarly unilateral. Secondly, that the recovery of the sense of taste and that of smell closely accompanied the removal of the neuralgic pain and of the anæsthesia, while the paralyses of ocular muscles remained unaffected. "The close connection," Dr. Anstie says, "between the sense of taste and the condition of the fifth nerve, is, of course, well known. But the apparently intimate connection between the state of the fifth nerve and the integrity of the sense of smell, in this case, stands on a different footing, and reminds us of the curious experiments of Bernard, in which he destroyed the olfactory nerves without materially impairing the sense of smell. I need not remind the Society, that in many lower animals the function of smell is entirely performed by the trigeminus, and it would seem that even in man the latter nerve at least retains an important office in connection with olfaction." Lastly, the case illustrates the close connection between an anæsthetic condition of sensory nerves to outward impressions, and the presence in them of neuralgic pain. Dr. Anstie has met with this association so frequently that he is convinced that neuralgia, so far from representing the opposite condition of sensory nerves to that which is represented by anæsthesia, is the expression of a half-paralytic condition of such nerves. The relation, he says, is aptly illustrated by the fact that the paralyzing agency of freezing cold produces intense pain before it obliterates sensation, and that during the recovery from frost-bite the same intense kind of pain reappears. J. H. H.

We shall next invite the attention of our readers to the surgical papers contained in the volume.

Art. I. *Cases of Parenchymatous Keratitis, associated with Acute Rheumatism.* By W. Spencer Watson.—A short abstract of this paper appeared in the Quarterly Summary of this Journal, in the number for January, 1871, p. 281. The corneal changes in the cases recorded were indistinguishable from those seen in cases of keratitis depending upon hereditary syphilis, and Mr. Watson considers it therefore a fair question to inquire whether the syphilitic corneal inflammation may not really be a form of rheumatic ophthalmia. In two of the four cases here narrated, the affected eyes were found, after the subsidence of the keratitis, to have become markedly myopic.

Art. II. *Case of Tetanus; Treatment by Ice to the Spine, Belladonna, Chloral, Wine, and Good Diet; Recovery; Interesting Variations in Temperature observed.* By John W. Ogle, M.D.

Art. III. *Injury to the Liver resulting in Abscess; Rupture on the 14th day, diffuse Peritonitis; Formation of a second Abscess, opening into the Colon on the 25th day; Interrupted Discharge of Pus by the large Intestine until the 112th day; Complete Recovery.* By John Harley, M.D.—Abstracts of both of these papers, the latter of which describes a case of extreme rarity and interest, have appeared in the Quarterly Summary of this Journal, the former at page 274, and the latter at page 263 of the number already referred to.

Art. V. *Cases of Optic Neuritis.* By Robert Brudenell Carter.—Three cases are narrated, and some interesting remarks appended as to the pathology of the affection known as optic neuritis. This condition, which Mr. Carter believes to be sometimes not an inflammatory one, is often present in cases of cerebral disease in which vision is not impaired, such cases not being usually brought under the observation of ophthalmic surgeons until the power of seeing is more or less interfered with. In no case which Mr. Carter has examined, has uncomplicated optic neuritis extended so far as to produce a visible effect on the yellow spot, and the blindness appears to be rather attributable to arrest of conduction, by interference with the function of the nerve tubules, than to any change in the percipient layer. The appearances seem to depend upon cell proliferation in the area of the disk, and there is no œdema of the retina, as there would be, if the current of blood in the central vein were at all seriously interrupted.

"The absence of œdema may, perhaps, show that the dwindling of the arterial and capillary circulation of the disk and retina is an effect of the atrophy rather than a cause; and that the process of wasting is not one of mere contraction of tissue. It seems to be quite certain that a state resembling optic neuritis, but with a greater degree of venous tortuosity, may be produced by growths within the cranium. My third case seems to show that the malady may have a traumatic origin; and I think the other two point to an alliance between optic neuritis and some unknown diathesis or constitutional cause; an alliance analogous to that which connects iritis with rheumatism and with syphilis."

Art. VI. *Cases of Skin-Grafting and Skin-Transplantation.* By G. D. Pollock. Communicated by the President.

Sequel of Mr. Pollock's Case of Extensive Burn healed by Skin-Grafting. By J. Warrington Haward.

Art. VII. *On the Transplantation of Portions of Skin for the Closure of large Granulating Surfaces.* By George Lawson.—Abstracts of these papers have already been published in the Quarterly Summary of this Journal for January, 1871, page 277. Mr. Lawson advocates the employment of grafts of considerable size, but Mr. Pollock believes the use of very minute portions to

be equally satisfactory, while it is certainly more agreeable to the patient. Mr. Haward's "sequel" reports the complete recovery of Mr. Pollock's patient, and points out the importance of multiplying the centres of new growth by repeated transplantations, so that the scar may thus more closely resemble true skin.

Art. VIII. *A Case of Vaccino-Syphilis.* By THOMAS SMITH.—The patient, a married man, aged 29, presenting no appearance of having previously suffered from syphilis, was vaccinated by four punctures with lymph taken directly from an apparently healthy infant, which, however, three weeks after its own vaccination, had an eruption on the head and buttocks, with "snuffles" and fissured lips. "Three of the punctures ran the ordinary course of vaccination pustules; the fourth pustulated, scabbed over, and at the end of three weeks or a month from the vaccination, its base began to harden, and the induration began to extend." This indurated spot assumed the appearance of a true chancre, and various secondary symptoms followed, though the characteristic glandular implication of syphilis was never very clearly marked. Improvement followed the use of mercury. A report upon this case is appended, signed by Messrs. GEORGE G. GASCOYEN, BERKELEY HILL, and GEORGE W. CALLENDER, who, though confirming the accuracy of Mr. Smith's observations, add that "the reporters, whilst they are of opinion that the ulcer on the arm and the eruption on the skin were of a syphilitic nature, would at the same time point out to the Society the possibility that one of the punctures may have been inoculated with syphilis accidentally from some outside source subsequent to the vaccination."

Art. IX. *Case of presumed Injury to the Ciliary Nerves of the Eyeball.* By ROBERT BRUDENELL CARTER.—A short abstract of Mr. Carter's paper appeared in the number of this Journal for April, 1871, page 581. From a supplementary note, dated June, 1871, we learn that "since the boy was presented to the Society, a new evidence of the shock to the posterior pole of the eyeball has been afforded by the gradual supervention of a considerable degree of myopia."

Art. X. *Syphilitic Nodes in an Infant.* By Dr. MARTIN PAYNE. Communicated by W. CAYLEY, M.D.—The most interesting point in this case is the fact that syphilitic lesions, which are usually of late appearance, were here observed in a child only ten months old. There were two large gummatous tumours (cellular nodes) in the cephalic region, and there had been others on the feet and hands. The first evidences of hereditary syphilis appeared when the child was two months old, his mother having, previous to his birth, miscarried once, and having had one child stillborn. The father presented no symptoms of syphilis, except a recurring sore throat, but the mother had had psoriasis, sore throat, rheumatism, and some affection of the eyes, the nature of which is not mentioned. The child's condition, both locally and generally, was much improved by the administration of mercury, cod-liver oil, and the syrup of the iodide of iron.

Art. XI. *A Case of Spontaneous Fracture of the Femur.* By ARTHUR E. DURHAM.—This is a case of much interest: the patient, a man about forty-four years old, after two falls, neither of which appeared to have done him any harm, began to suffer from obscure pains in the right thigh (at first supposed to be neuralgic or rheumatic), and one night while getting into bed was suddenly seized with acute pain, the limb becoming much swollen, and when subsequently examined, being found manifestly the seat of a fracture. "There was shortening . . . to the extent of about three inches, eversion of the foot, and inability to lift the limb. . . . Crepitus was readily detected, but . . . was more like that produced by cartilaginous surfaces than the hard, rough crepitus of

fractured bone." Extension by means of a weight was applied, while lateral support was afforded by the adaptation of a "leather-felt" splint, moulded to the pelvis and thigh, and under this treatment firm union was obtained in about four months.

"With regard to the true pathology of this case," adds Mr. Durham, "there is much room for doubt. My own impression is that the thigh-bone was damaged—evidently not broken, but probably severely bruised—at the time of the fall down stairs three months previously; that degeneration instead of repair, and absorption of the bony tissues without suppuration, gradually took place, until the bone became so thinned that it gave way under a strain so very slight that the sufferer was unconscious alike of the strain and of the result which ensued. . . . To one point I would venture to direct attention, inasmuch as it seems to me suggestive of discussion. The patient had suffered severely from worry, anxiety, and sorrow, and concomitantly from great strain upon the powers of the brain and nervous system. Now, some years ago, when working at the subject, I collected a very large number of detailed cases of mollities ossium or osteomalacia, and curiously enough the only particular as to possible causation in which all agreed was this—that, in every instance, in some way or other, the patient had been subject to great wear and tear and exhaustion of the brain and nervous system. The question of the relation between over-excitement and rapid waste of the nerve-tissues on the one hand, and degeneration, or, at any rate, deficient repair of the bony structures on the other, may at least be considered to open ground for discussion."

Art. XII. *Operations for Loss of a large portion of Male Urethra.* By Sir Henry Thompson.—The particulars of this case, which is one of much interest, have already been published in the Quarterly Summary of the number of this Journal for April, 1871, p. 577.

Art. XIII. *On the Nature of the so-called Congenital Tumour or Induration of the Sterno-Mastoid Muscle.* By Thomas Smith.—This affection consists in a swelling, sometimes appearing as an irregularly shaped oblong induration, occupying a considerable portion of the sterno-mastoid muscle, and evidently within its sheath; sometimes as an abruptly limited lump; and sometimes as two lumps with a distinct interval between them. The swelling is commonly very hard, and gradually disappears in the early months of infant life. Mr. Smith rejects the supposition that this so-called "congenital tumour" is either a new growth or a syphilitic deposit, and makes the very plausible suggestion that it is due to a rupture, either partial or complete, of the fibres of the affected muscle, giving rise to effusion of blood within the muscular sheath, and to retraction of the torn fibres. This view is supported by the histories of two cases of the affection in question, in each of which the lump was observed immediately after birth, the child having been brought into the world by forcible traction upon its legs.

Art. XVII. *Four Cases of Operation for Unusually large Calculi.* By W. F. Teevan.—The calculi in the respective cases weighed 4½ oz., 2½ oz., 5 oz., and 4½ oz., the ages of the patients being 74, 55, 32, and 65. In each case the incision was enlarged after the stone was grasped by the forceps, by making three or four successive cuts with the probe-pointed knife. The patients all recovered. Mr. Teevan asserts that the method of operating with a limited incision of the prostate "is usually followed by one of three disasters: death, impotence, or incontinence of urine;" but he gives no statistics to show the correctness of this opinion, which is contradicted by the general experience of lithotomists.

Art. XXI. *Cases Illustrating the Process of Occlusion in the Arteries after Acupressure; with its Relation to the Treatment of Surgical Hemorrhage, and as compared with Ligature and Torsion.* By Frederick J. Gant.—Mr.

Gant's observations confirm those of Messrs. Lee and West, as to acupressure, and of Mr. Forster, as to torsion—the first having been published in vol. i., and the latter in vol. iii. of the *Transactions*. (See Nos. of this Journal for October, 1869, p. 477, and October, 1871, p. 496.)

Art. XXIV. *Case of Removal of the Tongue for Cancer.* By HENRY LEE.—The four lower incisor teeth having been extracted, a double ligature was passed through the tongue, and the lip and lower maxilla then divided in the median line; the tongue was next drawn forwards by means of the ligature which transfixed it, and carefully dissected with curved scissors from the parts below. The base of the tongue was then effectually strangulated by repeated introductions of a nævus needle carrying a long ligature, of which three loops were left above the tongue and a free extremity at either end below; the loops being divided, there remained four ligatures, by which the tongue was firmly tied in as many portions. To give additional security a long, curved needle was now passed through the tongue in the line of the ligatures from side to side, and a strong ligature tied over it, and finally several turns of elastic ligature passed around the base of the tongue in the same way. The tongue was then cut off in front of the ligatures without any hemorrhage. The patient recovered. The following practical directions of Mr. Lee are worthy of attention:—

"Two sources of danger have been usually looked for after operations on the tongue; the first is hemorrhage, and the second, where a ligature is used, is the contamination of the blood in consequence of imperfect strangulation, as in the case published by William Hodgson. The ligatures applied in the mode described very effectually controlled the hemorrhage in the case related; and the use of the elastic ligature goes far, in my opinion, to prevent any circulation being restored in a part once strangulated. . . . The mode of feeding a patient, after such an operation, is worth consideration. It is desirable to leave the parts in the median line as quiet as possible, and a patient may be fed without in any way interfering with them. A gum-elastic catheter, or a feeder with a small nozzle, may be introduced between the cheek and the molar teeth, and any fluids so given will readily find their way between the teeth to the back of the mouth."

Art. XXVI. *Case of Complicated Stricture of the Urethra, treated by Mr. Syme's Operation for Impermeable Urethra.* By CHRISTOPHER HEATH.—The account of Mr. Syme's proposed operation for impermeable urethra (which we have always looked upon as one of the least happy of that distinguished surgeon's inspirations) is to be found in the 40th volume of the *Medico-Chirurgical Transactions*, p. 113. The proposed method consisted in introducing a large staff, grooved on its concavity, through a perineal wound (or fistula, if there were one) into the bladder, and then, with an ordinary "Syme's staff," forcibly perforating the urethral obstruction, carrying the beak of the smaller instrument along the groove of the larger into the vesical cavity; the operation was then to be completed by cutting outwards with a knife placed in the groove of the smaller staff, and introducing a full-sized catheter, which was to be allowed to remain two or three days. In the case reported by Mr. Heath, the operation, as above described, was modified by substituting for the perineal incision the introduction of a Holt's dilator, by which the stricture was split, and a No. 8 silver catheter then passed and tied in. We hope the successful issue of this case will not lead other surgeons to follow Mr. Heath's example, in adopting this modified form of the old-fashioned "forced catheterization" employed by Boyer and Mayor—a procedure long since condemned by the almost unanimous voice of the profession.

Art. XXVII. *Case of Retention of Urine from Impassable Stricture, treated by Filiform Bougies.* By W. F. Teevan.—The patient had suffered from stricture for seven years, and for about ten months had had retention of urine masked by overflow. Complete retention having supervened, Mr. Teevan, with some difficulty, succeeded in passing a very small filiform bougie, upon the withdrawal of which, urine began to flow, nearly three quarts being evacuated in the course of an hour. Gradual dilatation was subsequently employed, and the patient entirely recovered.

Art. XXXI. *Case of Encephaloid Cancer of the Femur, treated by Electrolysis.* By Lawson Tait. Communicated by Dr. Althaus.—This case is one of very great interest. The effect of the electrolytic treatment, which was not employed until late in the history of the case, was, except upon the first occasion, to afford complete relief from pain, for four or five days after each application, and to slightly diminish the size of the tumour. The intensity of the pain, before the application of electricity, may be inferred from the fact that it had been found necessary to administer hypodermically from 20 to 24 grains of morphia in each twenty-four hours. We quite agree with Mr. Tait that electrolysis is worthy of a fair trial in any case of cancer in which operative interference is not deemed advisable.

Art. XXXIV. *Case of Naso-pharyngeal Polypus, Brain Mischief; Operation; Death.* By J. Cooper Forster.—This also is a case of great interest. The patient was a young man, and the growth was at first supposed to be an ordinary nasal polypus of the firm variety; a considerable portion was removed by avulsion, the operation being attended with profuse bleeding, and the patient, after recovering from the effects of chloroform, complaining of intense headache, followed on the fourth day by aphasia, on the seventh by general convulsions, and on the twelfth by coma and death. Post-mortem examination showed general arachnitis, with pus in various parts of the membranes, cerebral softening in the neighbourhood of "Broca's convolution," and a large quantity of fluid in both lateral ventricles.

"Upon removing the bone about the anterior part of the base of the skull, we were enabled to see exactly from what part the growth arose. It occupied the left side, and filled the space between the greater and lesser wings of the sphenoid, the orbital plate of the frontal, and the cribriform plate of the ethmoid bone. From the nasal fossa it had extended by way of the sphenoidal fissure into the back of the orbit, but without damaging the optic nerve. The cribriform plate of the ethmoid bone was broken, and at its back part there was a small opening about a quarter of an inch in diameter, and a fracture extending forwards from the opening. Microscopically the growth consisted of small fusiform cells and stellate connective tissue, the body of the cells often much swollen, and containing tolerably large nuclei."

Mr. Forster accounts for the implication of the brain by supposing that the growth was adherent to the portion of bone which was broken, and where the small opening existed, and that the fracture was produced in the act of avulsion. In view of the fact that when, in injuries or diseases of the skull, the brain and membranes once become involved, all modes of treatment seem equally unproductive of benefit, Mr. Forster asks the question whether anticipatory treatment, by putting the patient under the influence of small doses of mercury, might not tend to avert the tendency to inflammation. We have long been in the habit, in such cases, of giving small doses of calomel in combination with opium, and, we think, with decided benefit; but to which drug the benefit is to be chiefly attributed may be a matter admitting of some dispute.

Art. XXXVII. *Cases Illustrating the Treatment of Herniæ Temporarily Irreducible.* By C. HOLTHOUSE.—Mr. Holthouse does not believe that the administration of opium in cases of strangulated hernia can do harm by masking the symptoms, but, on the contrary, regards it as a delicate test of the condition of the hernia, and of the necessity, or otherwise, of a surgical operation. If the almost universal experience of practical surgeons permitted us to believe that Mr. Holthouse is correct in the statements which follow, we should feel that the treatment of strangulated hernia was a much simpler matter than we are now obliged to consider it. "After the patient has been placed in bed," says Mr. Holthouse, "one grain of solid opium is given him, washed down with half a wineglassful of iced brandy and water; if the pill be retained, the obstruction is not invincible, the symptoms will subside, and the patient will recover; if, on the contrary, it be rejected, he should at once be brought under the influence of chloroform, one gentle attempt made to reduce the hernia by the taxis, and failing this, the operation of herniotomy performed."

Art. XL. *Case of Acute Disease of the Hip-joint, following the Introduction of a Tangle Tent into the Cervix Uteri.* By ALFRED MEADOWS, M.D.—This is a very curious case. Though the affection of the hip followed so quickly after the introduction of a tent into the neck of the uterus as to appear to have been directly caused by such introduction, careful examination both before and after death (which resulted from acute tuberculosis) failed to show any direct connection between the two. The patient had some time before received an apparently trivial injury of the hip, and it is probable that this, together with the tuberculous diathesis, which she undoubtedly possessed in a high degree, were the real causes of the articular affection; and that the apparent connection between this and the treatment of her uterine disorders was the result of coincidence alone.

Art. XLII. *On the Reduction of an old Dislocation at the Shoulder.* By GEORGE W. CALLENDER.—The patient was a man, aged 46, and the dislocation (downwards and forwards) was of one month's standing: in order to avoid any risk of injuring the axillary vessels, Mr. Callender employed the following manipulation:—

"The patient was placed under the influence of chloroform and, after rotating the head of the bone so as to break down adhesions, the arm was forcibly drawn upwards across the sternum, the elbow being raised almost to the level of the axilla. In this way the head of the bone was depressed. The trunk being fixed, the arm was then forced outwards, by pushing against the elbow, the forearm being flexed, and at the same time movements of rotation were made, the arm being occasionally lowered from the level of the axilla. Reduction was soon effected."

Though the present volume of the Clinical Society's *Transactions* does not seem to us to be quite as interesting as some of its predecessors, it nevertheless contains a good deal of useful material, and altogether forms a satisfactory part of a series which is already becoming one of very great value.

J. A., JR.

ART. XX.—*Saint Bartholomew's Hospital Reports.* Edited by Dr. ANDREW and Mr. CALLENDER. Vol. VII. 8vo. pp. vi., 266. London: Longmans, Green & Co., 1871.

THE seventh volume of the Saint Bartholomew's Hospital Reports is before us, and, in accordance with our custom, we shall present our readers with an analy-

sis of its contents, first inviting their attention to the medical papers, and then to those which are of special interest to surgeons.

Art. I. *The Shape of the Head, looked at from a Medical Point of View.* By S. J. Gee, M.D., Lond.—In this paper Dr. Gee directs attention to the pathological knowledge which may be obtained by a study of careful measurements of the head, and recommends the "cyrtometer" (described by the author in his recent work on Auscultation and Percussion) as a valuable instrument for obtaining the requisite measurements. The proper diameters for measurement are stated, and the normal size and shape of the head determined. The abnormal shapes are thus classified: I. Big heads: Macrocephali. i. Long heads: Dolichocephali. ii. Round heads: Cyclocephali. II. Little heads: Microcephali.

Dolichocephalus Dr. Gee believes to be always the result of increase in the solid contents of the skull and not in its liquid contents, and may be either absolute or relative; when relative the body is dwarfed, whilst the brain and the head remain of a size proper to the age of the child. Rickets, which stunts the growth of the body, may be called the cause of relative dolichocephalus, but when the dolichocephalus is absolute, Dr. Gee doubts whether the rickets is more than an accident likely to occur in a child with a great brain, who is often a weak child.

Hitherto, it has been difficult to understand why, in a rickety head, the sutures continue to be separated when the brain has shrunken in consequence of general marasmus, but a post-mortem which Dr. Gee recently made throws some light upon this subject. A baby, not wasted, but with well-marked rickets and craniotabes, died suddenly. The sagittal suture was half an inch across, and, before the scalp was removed, felt open throughout. But when the skullcap was taken off, it was found that the suture was filled up here and there by narrow ossa Wormii, which most effectually played the part of struts to keep the bones asunder. Dr. Gee supposes that, in these cases, the sutures open in consequence of progressive growth of brain and arrested growth of bone; ossa Wormii are formed; and then, when marasmus sets in, and the brain wastes, the bones cannot come together; compensatory hydrocephalus ensues, merely to fill up the space between the brain and the skull.

Dr. Gee has examined several dolichocephalic heads in older children who presented signs of past hereditary syphilis; he believes that it has no connection either with syphilis or arachnitis, that is to say, hydrocephalus, but that these children were rickety as well as syphilitic in their infancy—a combination common enough, and that the effusion found in the heads of syphilitic children who have died is compensatory and marastic. The interstitial encephalitis, which does occur in syphilitic infants, is free from meningeal lesions.

Cyclocephalus is characteristic of hydrocephalus. Microcephalus is apt to occur in connection with idiocy.

Art. III. *Four Cases of Hæmophilia.* By J. Wickham Legg, M.D., Lond. —The author defines hæmophilia to be an hereditary disease, manifesting itself during the first years of life, attended by a hemorrhagic diathesis, and a tendency to swelling of the joints, this latter a very important feature of the disease. This disease is more generally known under the name of the hemorrhagic diathesis, a name which is objectionable because it does not separate hæmophilia from many other diseases in which a hemorrhagic diathesis is present, such as scurvy, purpura, leucæmia, and jaundice.

The clinical history of these four cases presents nothing unusual; yet it is interesting from the comparative rarity of the disease and our ignorance con-

cerning its pathology. Billroth[1] thinks that the disease is probably due partly to an abnormal thinness of the arterial walls from lack of muscular tissue, and partly to deficient power of coagulation in the blood. This latter point has not been proved from the blood that escaped, for in the cases where attention was directed to this point the blood flowed like that of a healthy person. One of Dr. Legg's cases shows the curious rule of transmission through the women of the family to the boys, the women themselves remaining free from any trace of the disease.

The swelling of the joints which was observed in one of these cases is thus described: "When he was six years old, his left knee began to swell, and this joint has been swollen, off and on, ever since. The knee may keep well for three or four months; it then begins to swell, and will be painful and large for several months at a time, so that the boy is seldom free from some trouble with his joints. Sometimes as the swelling is disappearing in the left knee, it appears in the right, and *vice versâ*. Sometimes the ankles swell; but the left knee is the joint most commonly disabled. No swelling has been noticed in any other joints. No difference has been noticed in summer or winter, or in cold or wet weather."

Art. VII. *The Influence of Vaccine Inoculation on Guinea-pigs.* By THOMAS LEEDS.—In this paper are recorded briefly the notes of fifteen experiments made by inoculating guinea-pigs with vaccine virus. In some of the animals killed on the 75th day minute yellow patches were discovered in the liver and spleen, which, on microscopic examination, were found to consist of corpuscles, scarcely distinguishable from pus corpuscles. The experiments, however, did not give uniform results, and were inconclusive.

Art. VIII. *A Plea for the English View of the Nature and Pathology of Addison's Disease: in Answer to the most recent German Doctrine.* By H. M. TUCKWELL, M.D., Oxon.—In this article Dr. Tuckwell gives the clinical history and autopsy of a case of Addison's disease which was under his care, and the appearance of the supra-renal capsule is shown by a plate. Dr. T. criticizes the German view of its pathology as set forth by Rossbach[2] and contends that it has been fully refuted by the investigations of Wilks[3] and Greenhow.[4] The "English View" he sums up as follows:—

"Addison's disease is a special and peculiar affection of the supra-renal capsules, in which the adjacent ganglia and plexuses of the sympathetic system are generally involved.

"This affection has well-marked anatomical characters by which it can always be recognized, and is best defined as a chronic inflammation of low type, with a strong tendency to cheesy degeneration.

"It is one of the most deadly diseases with which we are acquainted, destroying life slowly by symptoms of distressing nervous exhaustion.

"It almost invariably gives rise to a discoloration of the skin and mucous membrane of the mouth, which ranges from a mere sallowness up to the deepest bronzing, the depth of colouring being apparently in direct proportion to the duration of the disease.

"It can be diagnosed by careful observation of its combined general and local symptoms, with as much precision as any other pathological change in the

[1] General Surgical Pathology and Therapeutics, translated by Hackley, p. 24.

[2] See No. of this Journal for April, 1871, p. 566.

[3] Guy's Hospital Reports, 3d series, vol viii. p. 1, and No. of this Journal for January, 1863, p. 165.

[4] "On Addison's Disease," London, 1866, and No. of this Journal for October, 1864, p. 533.

body. For there are peculiarities in its general symptoms which distinguish it from other unimportant neuroses; and there are peculiarities in the pigmentation which distinguish this from other unimportant discolorations of the skin.

"It may be capable of temporary relief by treatment, but is, with our present knowledge, absolutely incurable."

Art. X. *Observations upon the Action of Ipecacuanha and its Alkaloid Emetia. Part II.* By Dyce Duckworth, M.D., Edin.—Part I. of these investigations, it will be remembered, was noticed in the No. of this Journal for January, 1870, p. 200. The results obtained by Dr. Duckworth's further experiments may be summed up as follows: Ipecacuanha and emetia must be classed among the irritant emetics. The toxic effects of emetia are found to be similar whether the poison be given by the mouth or by the skin, and the degree of enteritis seems to be determined by the time allowed for the emetia to act. The sudden introduction of large doses of emetia produces rapid paralysis of the heart, and the effect upon the temperature is to lower it. Emetia exerts no important influence upon arterial tension.

In some clinical notes on the therapeutic value of ipecacuanha and emetia, the author shows their somewhat extensive range of applicability, and, among other noteworthy observations, we find it here stated that ipecac and rhubarb work together more effectually in relieving constipation than does either singly in the same case. This action, the author says, is well observed in the case of the compound rhubarb pill, five grains of which, if combined with one of powder of ipecacuanha, act as efficiently as ten or fifteen grains of the pill by itself.

Art. XIII. *Cases of Morbid Anatomy.* By Samuel J. Gee, M.D., Lond.—In this paper the author records several very interesting cases of morbid anatomy. Of these one is a case of *primary cancer of the liver* occurring in an infant five months old. The gland was enlarged and weighed forty ounces and a half.[1] The lymphatic glands at the hilum were unaffected. The lobes were studded with round masses of foreign growth, white, tough, and of all sizes, from the smallest possible, to that of a peeled walnut. The number of nodules is opposed to the view of primary cancer, but no cancer was found in any other part of the body. The importance of this case lies in the fact that the child was only five months old. This with one alluded to by Dr. Farre in his work upon "The Morbid Anatomy of the Liver," are the only cases occurring in children under two years of age known to be recorded. Dr. Farre's patient was three months old, but the liver affection was secondary to cancer of the retro-peritoneal glands.

The next case is one of *complete obliteration of the mouths of the hepatic veins*, and is of extreme interest. There is but one other like it believed to be upon record.[2] The specimen was taken from a male child aged 17 months, who, when fourteen months old

"seemed to have pain in his belly, and his whole body swelled. The swelling settled in his belly in a day or two, and passed away from the rest of his body. . . . When 16 months old, his belly was tapped, and 12 ounces of greenish serum (sp. gr. 1011) were let out. Serum continued to drain away from the hole, so that next day a canula was put in again, and 2 pints more were drawn off. The fluid gradully collected again. Nineteen days after the first tapping it was done again, and rather more than 2 pints flowed out. Five days after this he died, slowly exhausted, as it seemed. His urine was not albuminous."

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[1] The liver of a healthy child of this age would not weigh more than seven or eight ounces.

[2] See Virchow's Jahresbericht for 1867, vol. i. p. 226.

At the autopsy there was found no inflammation of the peritoneum. The liver was denser, tougher, and probably smaller than it should be, and its surface was slightly nodular. The gland was found to be congested, indurated, and fatty. The hepatic veins ended abruptly, "just short of entering the [vena] cava, being cut off from it by a thin membrane only. The lining membrane of the cava was perfectly smooth and natural, but where the mouths of the hepatic veins should have been there were shallow dimples, which had not at all the look of scars." The largest of the hepatic veins passed into the right and left lateral ligaments, and there anastomosed with greatly enlarged vessels contained in the ligaments, which, in their turn, communicated with large veins on the under surface of the diaphragm over the liver. The ductus venosus was not opened.

Dr. Gee frames the following hypothesis to account for the lesions above described. The liver was originally well formed and subsequently became cirrhosed; the cirrhosis seizing upon the capsule around the mouths of the hepatic veins more particularly. He deems this, for three reasons, to be the correct explanation:—

"First, an obliteration of the branches of the hepatic veins is known to be an occasional consequence of cirrhosis. Next, there were dimples in the vena cava, as if they were left to indicate where once the mouths of the hepatic veins had been. And, lastly, I should expect a congenital obliteration of the hepatic veins to be accompanied with a permanent obliteration of the ductus venosus, in a manner such as that whereby congenital closure of the pulmonary artery ensures a patent foramen ovale or septum ventriculorum."

Among the other interesting cases here recorded we may enumerate one of *hydatid tumour within the substance of the brain*, thirty cases of which have been collected by Dr. Bastian,[1] one case of *aneurism of the basilar artery*, and one case of *aneurism of the anterior communicating artery*. In proof of the rarity of aneurism of this latter artery, the author states that Lebert[2] could find only two cases of it upon record. Dr. Gee is evidently unacquainted with Dr. Hutchinson's elaborate study of intra-cranial aneurism, which was published in the second volume of the *Pennsylvania Hospital Reports* (1869), and in which five cases of aneurism of the anterior communicating artery are collected.

Art. XIX. *Notes of three Cases of Acute Yellow Atrophy of the Liver.* By DYCE DUCKWORTH, M.D., Edin., and J. WICKHAM LEGG, M.D., Lond.—The notes of these cases are valuable from the rarity of the disease. Frerichs,[3] it will be remembered, has tabulated thirty-one cases of the disease, and Dr. John Homans[4] twenty others. The cases here reported did not present any particularly noteworthy symptoms.

Art. XX. *Influence of Temperature on the Pulsations of the Mammalian Heart, and on the Action of the Vagus.* By T. LAUDER BRUNTON, M.D.—The influence of warmth in quickening the pulse has been long known, but the hypothesis of Budge and Liebermeister, that the quickness of the pulse in fever is due to the increased temperature of the body, gives to the physician an interest in the subject which he would not otherwise have. Dr. Brunton made several experiments on the effect of temperature on the rabbit's heart, and from these and the observa-

[1] Reynolds' System of Medicine, vol. ii. p. 498.
[2] Berlin Klin. Wochenschr., 1866, Nos. 21–42.
[3] Treatise on the Diseases of the Liver, vol. i.
[4] See No. of this Journal for July, 1868, p. 53.

tions of others on healthy and feverish men, he concludes that the quickness of the pulse in fever is mainly due to the higher temperature of the heart.

I. M. H.

We shall now invite the attention of our readers to those papers which are more particularly interesting to surgeons, and first to

Art. II. *Cystic Tumour of the Jaws.* By Alfred Coleman.—Mr. Coleman believes cysts of the upper maxilla to be more common than is usually supposed, and expresses the opinion (which, if we mistake not, is now generally entertained by pathologists) that the affection ordinarily described as "dropsy of the antrum" does not consist of an accumulation of the natural secretion of that part, but is an example of cystic formation, probably due to the irritation caused by the presence of carious teeth. The treatment recommended by Mr. Coleman is to make a free incision through the bony wall of the cyst, with a strong scalpel, and then induce suppuration by introducing a tent of lint moistened with a solution of carbolic acid: if, however, the tumour be connected with a tooth which is so much diseased as to justify its removal, extraction should be practised, when the tent may be introduced through the alveolus; or if the cyst should itself come away with the tooth, the use of the tent may be dispensed with. Mr. Coleman's views as to the true nature of the so-called dropsy of the antrum were, he tells us, first announced in communications which he made to the Odontological Society of London, in 1862 and 1863.

Art. IV. *On Degeneration of Bone.* By Edward Latham Ormerod, M.D., Cantab.—This paper may be looked upon as a sequel to one *On the Pathology of Fatty Degeneration*, published in the 4th volume of the "Reports," and noticed in the number of this Journal for January, 1869 (p. 190); the fortunate discovery that an available supply of degenerated bones can be obtained from lunatic asylums, has enabled Dr. Ormerod to complete his study of "fatty degeneration" (so-called), by investigating the pathological anatomy of this condition in the osseous tissue.

"In selecting mollities ossium as the best illustrative example of degeneration of bone, I do not mean to imply that the terms are exactly commensurate. True, that in mollities ossium the pathological changes of degenerated bone are displayed in the most extreme and striking forms; but the same changes are to be seen also under other circumstances. They may occur as the apparent effects of old age, or of anything, in fact, which brings on premature decay. On the other hand, the want of earthy matter, which has suggested the terms of mollities or fragilitas ossium, does not necessarily imply degeneration of bone, for this is an essential character of rickets, which is a disease of quite a different nature."

In rickets there is defective development, in mollities ossium there is premature decay. Dr. Ormerod has made most of his observations upon degenerated ribs, because (at least in the insane) the disease appears commonly to be further advanced in these bones than in other portions of the skeleton.

"Usually . . . a degenerated rib is a little larger and rougher than natural; it is flattened perhaps and misshapen from frequent fractures. It yields to the knife or breaks in the hand, all its material strength depending on a thin outer shell of bone; and the exposed surface is dark, wet, and greasy . . . The general arrangement of the interior of the bone is best displayed in a decalcified section. Here we see the central medullary cavity divided into unequal and irregular spaces, the largest being in close connection with the thin outer wall of compact tissue." This layer of compact tissue presents "all the ordinary features of healthy bone, only somewhat confused and on a very contracted scale. On the outer edge there are a great many simple cells, large, round, and

without any canaliculi at all. . . . . Then comes an irregular deposit of bone, everywhere thin, in some places entirely wanting, laminated concentrically to the long axis of the rib, and freely dotted with periosteal bone-cells. The red zone is occupied by Haversian systems, which are crowded together and cut into each other. This is the only really normal part of the bone, and the one on which its strength mainly depends. . . . . But as we carry the examination deeper into the next zone, the Haversian canals are seen to be larger, and the transparency of the innermost concentric rings of the Haversian systems becomes a very prominent feature. If we should happen to be using a section stained with magenta, we shall observe that these transparent rings take the dye more readily than the rest, an indication, I believe, of the removal of the earthy matter, with which this dye has little affinity. Another characteristic of this zone, though not limited within these bounds, nor indeed absolutely confined to degenerated bone, is a minutely granular appearance of certain districts, sometimes involving entire Haversian systems, to the exclusion of the usual fine converging lines of the canaliculi. . . . . The earthy matter has apparently not been removed, as these granular patches do not take the magenta readily. The only explanation . . . . is, that the canaliculi are irregularly obstructed by the presence of some substance in their interior. Just as the dentinal canals are in what dentists familiarly know as red teeth. . . . The cancelli . . . . are very sparingly dotted with bone cells of the medullary type. The new transparent deposit of bone which lines the large areolæ is still more scantily provided. Indeed, just here the laminæ are so thin, and so closely packed together, that there seems hardly to be any room for bone-cells."

As in the case of the soft tissues, the degeneration of bone is, according to Dr. Ormerod, not properly speaking "fatty" degeneration, though it may be followed by fatty infiltration. The bone-cells are not liable to the occurrence of the latter condition, and the amount of fatty matter is probably proportionably less in the medullary mass of a degenerated, than in that of a healthy bone, the absolute increase or decrease depending more upon the shape of the bone than upon the degree to which degeneration has occurred. "Such fatty infiltration as there is, occurs in the interior of the enlarged Haversian canals. Here in the thickened coats of the vessels, and around them, we find abundance of minute oil globules, here is the source of the loose fatty matter which crowds the field of the microscope. The singular opacity of fragile bone, however, does not depend so much on the presence of adventitious fatty matter as on a molecular change."

Dr. Ormerod's excellent paper is illustrated with a plate containing five figures, and with several wood-cuts.

Art. V. *On the Treatment of Diseased Joints by Rest and Extension.* By W. S. SAVORY, F.R.S.—This is a paper of considerable interest and of great practical value: in the case of the hip-joint, Mr. Savory recommends the employment of the long splint and weight extension apparatus, with which American surgeons are so familiar; and in the case of the knee-joint, a hollow metal splint made to embrace the back and sides of the leg and thigh, with a foot piece, a hinge-joint at the knee, worked either by a screw passing between the thigh and leg pieces behind, or by a rack at the side; extension is made by means of another screw acting either through the foot piece or at the knee. Mr. Savory's remarks upon the advantages of combining rest with extension, in the treatment of joint diseases, rather than relying upon either rest or extension alone, appear to us eminently judicious, and we quite agree with his condemnation of the "fashion of the times" which aims to "dispense altogther with means [of treatment], however useful and advantageous they may be, if they can be shown to be not absolutely necessary." As Mr. Savory justly remarks, many things may not be absolutely essential to success in treatment, and yet render the

treatment much safer and much more certain to succeed than it would be if they were omitted.

Art. VI. *A Case illustrating certain Nervous Diseases.* By Sir James Paget, Bart., F.R.S.—This short paper gives an account of a very curious case. The patient was a gentleman aged twenty-eight, who, at the age of twenty, being subject to frequent "colds," was advised (on the "hardening" principle of which patients' non-medical friends usually think so highly) to employ cold shower-baths, and as the result of eight days' trial of this remedy, became the subject of a remarkable disturbance in the blood-supply of his feet, at first the left, but afterwards the right foot being the one principally affected.

"Whenever he walked slowly for so much as half an hour, or quickly for so much as ten minutes, the foot or feet became 'dead,' that is cold and numb, and (as he always expressed it) white, like marble. They thus changed much more quickly in cold than in warm weather, and often changed in the same manner during cold weather, even when he was at rest. With the deadness he always felt aching, and sometimes severely aching pains at the side of the foot, and sometimes half way up the leg. This state would continue so long as the patient continued walking; but when he rested, it would in a few minutes change, and the foot, or feet, would become flushed, ruddy, and hot, with overfilling of the veins, and the pain would slowly subside."

This uncomfortable condition, which was brought about in eight days, proved utterly rebellious to all kinds of treatment, having persisted for eight years when the patient came under the author's observation.

Sir James Paget uses this case as an illustration of several points of practical interest. In the first place, it furnishes a striking example of the effects of the shock of cold, which is sufficiently often the cause of injury "to suggest more caution than is commonly used in its employment."

"Few things," adds the author, "hinder recovery from old injuries of joints more than the use of the cold douche in any but very robust persons. And I have known a case in which a patient, who was healthy, except in having a too vivid nervous system, became, after being once submitted to a severe cold douche on his back, the subject of all the chief symptoms that commonly follow railway concussions of the spine."

The curious disturbance of the vaso-motor nervous system in this case, suggests the possibility of a similar condition in parts which are not exposed to view, in cases of spinal irritation, or so-called hysteria, and in parts which, though apparently healthy in structure, are morbidly sensible of fatigue.

Finnally, this case shows that "although the vascular and nervous conditions of a part are always disturbed in inflammation, they constitute only a subordinate part in that morbid process. They may be very often and far removed from their normal state, into one like that which they assume in inflammation, and yet no change of structure may ensue in the tissue-elements around them."

Art. IX. *On the History of the Treatment of Congenital Club-foot.* By Holmes Coote.—This short paper gives an interesting account of the early attempts to remedy the deformity of club-foot, and particularly of the modes of treatment employed before the introduction of tenotomy.

Art. XII. *On Ranula.* By W. Morrant Baker.—After referring to the opinion formerly entertained as to the nature of ranula, viz., that it consisted in a dilatation of the duct of the sub-maxillary gland, and showing, by quotations from several recent works on surgery, that this doctrine is still not without advocates, Mr. Baker gives several cases in support of the view that the disease in question is an example of independent cystic formation, and sums up "the

present evidence against the existence of a connection between ranula and dilatation of Wharton's duct," as follows:—

"1. There is no positive evidence of any kind, that obstruction of Wharton's duct accompanies, or is connected with, the formation of a ranula.

"2. There is abundance of proof that in cases of ranula, Wharton's duct is usually quite free; while, very commonly, saliva may be seen flowing from the duct, without any accompanying alteration in the size or other characters of the tumour.

"3. The fluid of a ranula is very unlike saliva, which may have been seen, a moment before the tumour was punctured, possessing its ordinary qualities of colour and consistence, as it escaped from the sub-maxillary duct.

"4. The fluid contained in the common form of ranula is hardly or not at all to be distinguished from that of certain cysts in parts of the body in which no salivary glands exist.

"5. The symptoms of known dilatation and obstruction of Wharton's duct are altogether unlike those of ranula."

Art. XIV. *On the so-called "Serpent Teeth."* By GEORGE W. CALLENDER, F.R.S.—The term "serpent teeth," Mr. Callender tells us, is the popular expression to denote torsion of one or more of the incisors, usually of the upper maxilla. This deformity is briefly mentioned by systematic writers on dental surgery, but no definite explanation seems to have been given of the mode in which it occurs. After referring to the development of those parts of the upper maxillæ which are connected with the incisor teeth, Mr. Callender proceeds to apply the facts which he has set forth, in the following summary:—

"1. If the incisor process is weak, or retarded in the time of its development, it has less than the normal control over the forward growth of the intermaxilla, and the alveoli grow forward and the teeth project in proportion.

"2. If the incisor process is stunted, it grows in and narrows the palate. By side pressure on the posterior portion of the intermaxilla, it forces it in, with a twist on its axis, and the sockets growing under the influence of this twist the incisor teeth suffer torsion. The temporary teeth may show little of this malformation, whilst the permanent teeth, the sockets of which are more closely related to the solid fabric of the intermaxilla, are those which suffer.

"3. In the treatment of the extreme malformation, cleft palate and harelip, when the incisor process has aborted, great care should be taken by the operator in pressing back the intermaxilla not to give it a lateral twist. . . .

"4. When the incisor process aborts, the incisor teeth are apt to suffer from imperfect lodgment, the protecting alveolar plates from the incisor process being absent; the lateral incisors are more likely to fail than the central, the latter having their sockets most largely formed by the central part of the intermaxilla.

"5. In the ossification of the central part of the intermaxilla, plates of bone are formed one above the other, those in front being placed horizontally, leaving intervals between them. This arrangement may account for the occasional horizontal position of a permanent incisor, the direction of its socket having been determined during the early steps of its development, and may also explain the chance formation of an extra chamber for a supernumerary tooth.

"6. If the labial surface of an incisor is twisted in (towards the middle line), it is due to pressure from the opposite side, the maxillary arch having been forced in from imperfect development of one of the incisor processes—the central incisor is the tooth usually distorted in these cases."

Mr. Callender's paper is illustrated with a lithographic plate containing six figures.

Art. XV. *On the Treatment of Cleft Palate in Infants, and on the Cure of Clefts of the Hard and Soft Palates by Operation under Chloroform.* By THOMAS SMITH.—This is perhaps the most important surgical paper in the volume,

and forms a most valuable contribution to the literature of staphyloraphy and uranoplasty. Since the publication of Mr. Smith's paper in the 51st volume of the *Medico-Chirurgical Transactions* (see number of this Journal for April, 1868, p. 541), he has operated in forty cases, which are here given in detail, with a full account of his mode of procedure, and an excellent practical commentary upon the whole subject of cleft palate and its treatment.

Mr. Smith has kept a record of 125 cases in all, in 16 of which the soft palate alone was involved, in 59 the soft and part of the hard palate, and in 50 the whole palate, soft and hard. The sexes are pretty equally represented in these figures, 59 patients having been noted as boys and 61 as girls.

The treatment in newly born infants consists merely in securing a proper amount of nutriment to the patient, the author agreeing with Mr. Pollock in considering operative interference inadmissible at this age:—

"The operation in infants, according to my experience, is unnecessary, and, as one gathers from published accounts, is much more dangerous to life and much less likely to succeed than if performed at a later period. Even if the performance of the operation could be justified on the score of necessity, one can scarcely imagine that in the case of an infant seriously atrophic from deficient nutrition the proceeding would be likely to succeed, or would add to the infant's chances of ultimate recovery."

If the child cannot take the breast, it may be fed with a spoon, with a feeding-bottle provided with a thin leaf-like sheet of India-rubber attached above the teat, so as to occlude the aperture in the palate (as advised by Messrs. Ramsay and Coles), or even with a feeding-bottle having a teat with a large hole through which the milk can be safely poured a little at a time into the pharynx.

The age at which operative measures should be instituted cannot be fixed by any general rule, but probably two or three years old may be taken as the minimum, while it can seldom be necessary to postpone the operation beyond the age of six.

With regard to the operation itself, Mr. Smith recommends (herein differing from Mr. Holmes) that, as a rule, the whole cleft, both of the hard and soft palate, should be closed at one sitting:—

"The length of the cleft adds little to the difficulty of the operation, nor does it diminish from the chance of cure. The highly arched palates, where the margins of the cleft are nearly vertical, are usually easily remedied, and they require for their closure less skill and less division of the soft parts than those clefts that are wider, where the palatine arch is flatter, the sides being more horizontal."

Mr. Smith recommends the use of several special instruments, the most important being a gag which is slightly different from that which he at first employed, a tubular needle modified from that used by Sims for vesico-vaginal fistula, and "suture scissors" which "quickly seize, divide, and withdraw a suture with one and the same movement." Horsehair and fine silver wire are recommended as the best materials for sutures. For separating the muco-periosteum, in the operation on the hard palate, Mr. Smith recommends ordinary aneurism needles, or a "Holden's director," as preferable to the raspatories ordinarily used, these being too large and thick for children. The point at which union is most apt to fail is at the junction of the hard and soft palate.

"It usually requires some care and patience to free the muco-periosteum and the fascia of the velum from their continuous attachment at this spot, and when much difficulty occurs in bringing the cleft together, I have with advantage, after making the lateral incisions, passed, as a supplementary support, a silver wire through these incisions and behind the new palate, so as

to draw together the margins of the cleft and relieve the tension on the sutures."

The after-treatment is very simple: no warm food should be given until the day after the operation, and no substances requiring much mastication for the first fortnight; the patient should, if practicable, be kept in bed during the first week, and the sutures should not (in the case of children under ten) be removed for three weeks or a month, and then, if the patient be turbulent, with the aid of anæsthesia.

Mr. Smith concludes his valuable paper with details of the forty cases in which he has operated since the publication of his communication to the Royal Medical and Chirurgical Society, which has already been referred to.

Art. XVI. *On Tubercle in the Eye.* By B. J. VERNON.—Mr. Vernon questions the desirability of retaining the expression "tubercle in the eye," as though the occurrence of tubercles in the choroid in cases of acute tuberculosis is undoubted, yet the anatomical features of these cases scarcely differ except in degree from those of suppurative choroiditis, and the diagnosis can hardly be made except by ascertaining the history of the case and by noting the presence of tuberculous disease in other parts of the body.

Art. XVII. *On Excision and Abscission of the Eyeball.* By B. J. VERNON.—In this paper is given a fair estimate of the comparative advantages and disadvantages of excision or enucleation of the entire eyeball, and of mere abscission of its anterior portion.

Art. XVIII. *Report of the Ophthalmic Department.* By HENRY POWER, M.B., Lond.—The ophthalmic wards of St. Bartholomew's Hospital have been open for the reception of patients only since October, 1870, and this is therefore the first report of the work therein accomplished. In the list of operations we observe 26 for cataract, 8 for capsular opacities, 26 for strabismus, and 37 iridectomies. Several cases of interest are narrated at length, and one (a case of hemorrhage into the substance of the retina, connected with disordered menstruation) is illustrated with a handsome chromo-lithographic plate. The following remarks upon the purulent ophthalmia of children are worthy of attention:—

"We may, I think, reasonably presume that the pus-globules discharged in such vast quantities in this affection are derived from two sources, partly and chiefly from the proliferation of the cells of the conjunctival mucous membrane, but partly also from the escape of the white corpuscles of the blood through the enlarged and congested capillaries and smaller arterial and venous trunks. The application of astringents would appear to be the proper remedy for the arrest of both processes, for by constricting the bloodvessels the supply of pabulum serving for the rapid division and multiplication of the epithelial cells would clearly be at once reduced, whilst, on the other hand, the number of the white corpuscles escaping from the vessels would certainly be diminished. Practice, in this instance at least, while it long preceded, is in full accordance with theory. . . . There would appear, however, to be some other condition present besides mere congestion of the bloodvessels, since not only do we meet with many other forms of conjunctivitis where intense redness is present, whilst the amount of discharge is comparatively small, but it is common to find that the due application of astringents temporarily checks the formation of matter, though it recommences again, and may even reach a higher point than before. On considering these circumstances it appeared to me to be probable that, since in most cases of purulent ophthalmia some kind of contagion was present, if this poison could be neutralized or killed, the progress of the disease would be stayed. With this in view, I determined to try and compare the effects of carbolic acid, chloride of zinc, and Condy's purple fluid

[a solution of the permanganate of potassa], both alone and in combination with alum lotion."

The carbolic acid, in such proportions as were considered safe, was found inert, and the chloride of zinc rather injurious than beneficial.

"The results of my experiments with Condy's fluid, however, have been of a much more satisfactory nature. . . . . The best proportion was found to be 100 minims of Condy's dark fluid to about 8 ounces of ordinary alum lotion, containing from two to four grains to the ounce, and the mode of employing it is as follows: The eyes should first be carefully washed with a jet of cold water to clear away the collected muco-purulent flakes; an examination being made, about an ounce of the Condy's fluid is then to be injected, and the mother directed to drop in a few drops every hour."

Art. XXI. *Second and concluding Report concerning a Case of "Anomalous Affection of the Nervous System in a Boy."* By Luther Holden.—We have here the sequel of a very remarkable case, the particulars of which were fully given in the third volume of the Reports. The patient had at the side of the neck a small tumour or "bump," pressure upon which gave rise to most curious nervous symptoms, a similar sequence of symptoms moreover occurring at intervals without any apparent cause. The tumour was removed by Mr. Holden with the gratifying result of effecting a complete cure. "The tumour was composed of fat. The most careful examination could detect nothing to account for its singular sensibility, beyond the ramifications of a slightly hypertrophied cutaneous nerve."

The present volume contains, as usual, elaborate statistical tables, which are of great value, and which reflect much credit upon their compilers, Dr. W. Ainslie Hollis and Mr. J. Astley Bloxam, the medical and surgical Registrars. It also contains an index, which, if not very elaborate, is at least better than none. Upon the whole, we think the volume is a satisfactory one, and compares very favourably with those which have immediately preceded it. J. A., Jr.

Art. XXI.—*The Manchester Medical and Surgical Reports*, October, 1871. Editors, S. Messenger Bradley, F.R.C.S., Walter Whitehead, F.R.C.S. Ed. Vol. II. pp. xxiv., 216. Manchester: J. E. Cornish, 1871.

This second volume of the *Manchester Reports* contains a large preponderance of surgical articles, short abstracts of which we lay before our readers.

The first paper is by Mr. Thomas Windsor, *Upon the Treatment of Wounds of the Eye by Suture*, which injuries, when extending deeper into the eyeball than the conjunctiva, were formerly left to nature, but for the cure of which sutures have been used by several authorities, in later years. They were first adopted in experiments and operations upon the cornea, having been recommended by Dr. Williams, of Boston, in flap extraction of cataract, while Dr. O. D. Pomeroy, in vol. lxviii. of the *Boston Medical and Surgical Journal*, reported their successful application to an incised wound of the sclera with coloboma. In confirmation of this practice Mr. Windsor narrates a case of slowly healing incised wound of the sclerotic, half an inch in length, where he introduced a suture with the happiest results. He also reproduces two similar cases reported by Mr. Bowman, together with one occurring in the practice of Mr. Lawson, and in an unpretending way the paper is a pleasant and instructive one upon this interesting subject.

W. C. WILLIAMSON, F.R.S., Consulting Surgeon to the Manchester Institution for Diseases of the Ear, furnishes a desultory article on *Some of the Difficulties encountered in the Diagnosis of Aural Diseases.* He first gives an interesting sketch of the comparative anatomy of the ear, and shows its progressive development in the ascending scale of animal life. The object of the writer seems to be to prove that the only aid we possess to the physical diagnosis of diseases of the ear, further than simple vision, is the otoscope, and that therefore much of our treatment of diseases of the internal ear is necessarily empirical; and that, consequently, the credit of aural surgery will be best maintained by frankly admitting that we can only rationally treat diseases of the Eustachian tube, external ear, and meatus auditorius externus. No attempt is made to clear up the difficulties said to exist further than to dissuade beginners from dependence upon books which give details of diseases which have not and cannot be demonstrated, and that tend to discourage the inquirer when he fails in practice to recognize the niceties laid down therein. The paper can only be regarded as a contribution to our knowledge from the fact of its attempting to draw the line between what we do and what we do not know, and viewed in this light it is possessed of practical value.

The paper next in order, which we are told was read before the Surgical Section of the British Medical Association at its meeting in Plymouth last year, prior to the address of Mr. Lister upon the same subject, is on *Antisepticity in Surgery*, by EDWARD LUND, F.R.C.S. Its author considers that the antiseptic method as now applied to the treatment of wounds is but the development of the old idea of the importance of preventing putrefactive change in the discharges. The objects aimed at are the prevention "of constitutional disturbance, the protection of the surface from loss of substance by sloughing or ulceration, and the saving of strength by the non-secretion of pus, or its formation in harmless quantities." It is admitted that the treatment recommended retards the healing of wounds, but the resulting cicatrix is thought to be more flexible from the smaller amount of fibrinous deposit, which depends upon the less inflammatory activity permitted throughout, and that in consequence of the small quantity of fibrin effused, there exists less tendency to contract. While Mr. Lund has had most experience with carbolic acid, he thinks many other volatile substances may be used with benefit, the theory being that they keep the wound in an atmosphere in which deleterious germs cannot exist. In conclusion, Mr. Lund thinks that the carbolized muslin dressing, as now advised by Mr. Lister, furnishes the best means of treating wounds antiseptically yet introduced.

W. H. BARLOW, M.D., Hon. Medical Officer General Hospital and Dispensary for Sick Children, Manchester, contributes the next surgical article, which has for its caption *On the Practice and Rationale of Skin-Grafting*, and reference is made in it to some cases treated by the method introduced by Messrs. Reverdin and Pollock. Pieces of skin as large as peas are excised by a bistoury and carefully spread out upon the granulations, care having first been taken to wipe the latter dry, and the grafts retained in position by a strip of plaster. Not only did cicatrization proceed from the islands of skin thus obtained, but the edges of the sores were observed to take on increased activity, which fact Dr. Barlow thinks may be accounted for on the supposition that, being no longer obliged to keep up the entire circulation in the cicatrizing tissue, the original local vitality of the part is expended on the repair of the periphery of the ulcer. Dr. Barlow concludes his contribution by pleasing anticipations of future triumphs to spring from this new method.

Mr. Windsor next writes *On the Treatment of Gonorrhœa by the Urethral*

Douche, and holds that if the inflammation can be thoroughly treated while it is confined to the anterior portions of the urethra, the attack will be cut short. For the accomplishment of this end Mr. Windsor thinks an easy and satisfactory method is presented in the douche, and he speaks of attempting to use a double catheter, but does not seem to be acquainted with the article on the same subject by Dr. Addinell Hewson, contained in the second volume of the *Pennsylvania Hospital Reports*, in which his views are anticipated nearly three years. The same author also presents a case in the succeeding paper *On the Use of Croton Oil in Old Opacities of the Cornea*, in which that irritant applied topically in a diluted form proved of much service, and was followed by no ill consequences.

One of the editors, Mr. S. Messenger Bradley, contributes some *Notes on Syphilis*, in which he regards the disease in question as zymotic, and maintaining the unity of the syphilitic poison, holds that the varieties observed depend upon the varying tissues in which the specific germs are deposited. He appears to mistake the ground occupied by dualists, for while they are obliged to admit that in some cases it is impossible to say whether a sore is a chancre or a chancroid, they yet maintain that the difference does exist, and will be manifested in the results; they contend further that the distinction is of value, as it can generally be recognized, thereby rendering possible a fairly certain prognosis as to whether secondaries may or may not be anticipated. All this is in great measure admitted by Mr. Bradley, whose distinctive belief is in *unity of species*, with varieties which, in the vast majority of cases, "breed true," but sometimes interchange. Some experiments upon the lower animals are adduced which will hardly be considered conclusive by dualists, though they may serve to arouse the mawkish sentimentality of those whose sympathies are keenly alive to the moral rights of the brute creation, who will doubtless regard such experiments upon guinea-pigs and kittens as not only cruel but positively immoral. The peculiar features of hereditary syphilis are briefly referred to, and an interesting contrasted parallel between the lesions of secondary and tertiary syphilis occupies considerable space. Several illustrative cases are cited, and valuable hints upon treatment, with a recital of the ways in which we can vary the use of the simple means at our disposal, conclude the paper, which, though somewhat indefinite, is pleasantly written, and will prove instructive to those who have not devoted time to the special study of this subject.

The next two contributions, severally entitled *Historical and Critical Sketch of the various Methods of treating the Pedicle in Ovariotomy*, and *Five Cases of Ovariotomy performed at St. Mary's Hospital, from January 1st to September 18th*, 1871, are from the pen of the same writer, D. Lloyd Roberts, M.D., F.R.C.P. Lond. The first, as its name indicates, gives an account of the multiform treatment to which the pedicle has been subjected since the first introduction of the operation by Dr. McDowell of Kentucky. The conclusions arrived at are, that no one method is universally applicable, but that some form of clamp should be used wherever it is allowed by sufficient length of pedicle, while in those cases where we have short, fleshy, and vascular stumps to deal with we must rely upon ligatures cut short, and the actual cautery. The latest forms of clamps are referred to, and wood-cuts of most of them given. Dr. Atlee's new clamp, described in the number of this Journal for April, 1871, is spoken of in terms of approbation, though Dr. Roberts has not yet tested it in practice. The whole subject is comprehensively treated, and the views expressed are fair and apparently unprejudiced. The second paper contains the history of five successful cases of ovariotomy, in which the age varied from twenty-four to forty-nine years. The clamp was used four times; once the

pedicle was secured by ligatures and the ends cut short; three of the tumours were multilocular, one unilocular, and one fibro-cystic; they weighed respectively 23, 19½, 21½, 27½, and 47 pounds. While the cases are well told, their number is too small to draw deductions from, in view of the so much more extended series of observations furnished by others.

Cases and Notes on the Surgery of Prolapsus Uteri and Elongation of the Cervix, by WALTER WHITEHEAD, F.R.C.S. Edin., is an article which partakes largely of the nature of a lecture. Its author regards the utero-sacral ligaments as the principal supports of the uterus, and thinks that sudden or continued pressure of the abdominal muscles and viscera is the general cause of prolapsus, while elongation of the neck of the uterus is in great measure dependent upon continued traction of the primarily prolapsed bladder. The conclusions arrived at are that no operation will be successful when we cannot diminish abdominal pressure. Some cases are appended which Mr. Whitehead thinks may *primâ facie* appear triumphs of obstetric surgery, but in consideration of the risk run and the occasional return of the mischief, he concludes that an operation should be reserved for extreme cases in the failure of all other remedies. The cases reported are fifteen in number, nine being instances of amputation of the cervix, and six of elytrorrhaphy. The peritoneum was opened in one operation of each class without injurious results, while death from pyæmia followed one operation of elytrorrhaphy.

Clinical Memoranda, by J. THORBURN, M.D., Physician to the Manchester Southern Hospital for Women and Children, comprise some notes, rendered imperfect by the illness of their author, on vascular tumours of the urethra, polypoid uterine growths with double attachment, and ovarian tumours, occurring in the practice of the Southern Hospital. In our opinion, notes admitted to be imperfect, and therefore not open to criticism, are hardly worth publishing at all; but in these times not only is the Horatian rule unappreciated or unknown, but nine *days* are scarcely elapsed ere each one rushes forward with a few more pages covered with print as his contribution to an already redundant literature.

Mr. ANDREW BOUTFLOWER, late Senior House Surgeon to the Manchester Royal Infirmary, reports a case of a *Foreign Body in the Urethra and Bladder*, in which Mr. Heath (not Christopher) removed a piece of the stem of a sage plant, three inches long, from the prostatic urethra and neck of the bladder by lithotomy, the offending substance having been introduced by the patient himself. *A Case of Dislocation of the Hip reduced Eleven Weeks after Injury*, by the same surgeon, is also reported by Mr. BOUTFLOWER. The head of the femur rested upon the dorsum ilii, and manipulation proved successful.

Mr. LUND contributes some really valuable *Suggestions for a ready Method of recording Surgical Cases in Hospital Practice*, which will be read with interest by hospital surgeons, who in general have encountered the difficulty which Mr. Lund attempts to obviate. The papers recommended are single leaves with printed headings which vary according to the taste of the reporter, who, striking out those which do not suit, arrives at the characteristics of the case by a process of exclusion. The age, temperament, social condition, state of the urine, termination, and numerous other points are very easily noted in this way. Mr. Lund further advises a system of contractions which appears to be a good one, though individual preference generally decides this in accordance with personal fancy. Lithographs are given, without which we fear the exact character of the special method recommended by Mr. Lund will hardly be understood. We do not know that the ideas are new, nor is such a claim made

for them, but the article is well considered, and will repay perusal by those interested in the subject.

The last paper in the volume, styled *Statistics of English Surgery in Public Medical Charities for the year* 1870, is by the EDITORS, and shows a laudable desire on their part to collect statistics which will carry weight by their number and by the sources from which they are derived. We have long felt that small masses of results are so easily and so often manipulated to suit the views held by their compilers that they are very far from being conclusive arguments. Figures naturally, it is true, do not lie, but that in an abnormal condition they, as well as facts, may become outrageously mendacious, is patent to all. We regret, with Messrs. Bradley and Whitehead, that they have not met with a more general reponse to the inquiries they have set on foot. The operations tabulated are, Amputations, Immediate Dilatation of the Urethra, Ligation of Arteries, Lithotomy, Lithotrity, and Ovariotomy, but the numbers are too few to be of material value.

In bringing this notice to a close, we can only regret the few cases contained in these reports and the evident difficulty of procuring any materials for an annual volume which appears to prevail in Manchester as on this side of the Atlantic. With hesitancy we would propound the question: is it advisable to publish a book every year whether there is anything to put in it or not? We will be answered, of course, that a great city, as Manchester, furnishes ample material if it were only worked up, and of this we have no doubt; but if men cannot be induced to work it up, the fact remains that valuable material for annual reports is very hard to get. S. A.

We shall now invite the attention of our readers to the medical papers contained in the volume.

On some of the Advantages of Hydrotherapeutics. By THOMAS LEEDS, Lecturer on Physiology at the Sheffield School of Medicine.—The author in writing this article is animated by the desire to draw the attention of the profession to the study of hydrotherapeutics as a part of the system of medicine; and is convinced, not unnaturally, that when a patient at a water-cure establishment is perpetually exposed to sitzbaths, packs, cold shallows, spinal douches, rainbaths, "and so on," at various times and temperatures, what change may occur in him is to be attributed not alone to change of life, fresh air, and the surrounding scenery. After describing the more important forms of water applications, the author relates several cases which were much benefited by having submitted to them; such cases as are, however, not unfrequently benefited by good food, fresh air, and exercise.

Case of Tubercle of the Cerebellum. By HENRY SIMPSON, M.D., Physician to the Royal Infirmary, Manchester.—This is a case related to illustrate some of the many difficulties lying in the way of positive diagnosis in certain cases of lesion of the brain. A lad of fourteen was admitted to the Manchester Infirmary for treatment of a condition dating back but five months, at which time he was attacked by severe, shooting, intermittent pain in back of the head, most felt on the left side a little above the superior curved line of occipital bone. Vomiting soon followed, with loss of appetite and constipation. He had always been healthy, and there was no history of any right to a constitutional taint of any kind.

Six weeks before admission his "eyes became strange, and he could not walk or talk so well as before." To these symptoms by degrees were added, gradual loss of sight, increased impairment of speech, and a want of co-ordination in muscles of locomotion. There was also some paralysis of left side of

face, and of the internal recti of both eyes; the paralysis of the recti advancing with the amaurosis which gradually increased. The ophthalmoscope gave proof of the presence of "increased pressure within the cranium due either to effusion or a tumour."

Emaciation set in after a time, but slowly. The temperature never rose to 100°, and fell regularly a degree or two in the evening. There were at no time convulsions.

Death occurred four months after admission. At the post-mortem examination the brain alone was examined. The veins of the dura mater were rather turgid. The visceral arachnoid was slightly thicker and more opaque than normal, especially about the vertex. At the base there was a considerable quantity of serum, and some lymph, principally deposited about the optic commissure and backwards to the anterior margin of the pons. There were no miliary tubercles to be discovered in any part of the membranes. The whole brain was somewhat soft, but otherwise the hemispheres seemed healthy as far as the level of the lateral ventricles, which contained several ounces of turbid serum. The septum was destroyed and the walls of the ventricles were softened so as to be almost diffluent. The venæ Galeni were very turgid. The posterior part of the pons and medulla oblongata was very much softened. Both lobes of the cerebellum were softened, and each contained numerous yellow tubercular masses varying in size from a pea to a marble. The right lobe contained a greater number than the left.

Description of a Melanometer. By W. FRANK SMITH, M.B. Lond., F.C.S., Physician to the Sheffield General Infirmary.—In this paper the author describes a simple instrument devised by him for the purpose of measuring rudely the amount of uromelanine present in any specimen of urine. Uromelanine being formed by oxidizing urochrome, which, in turn, is, according to Dr. Thudichum, a product of hæmatocrystallin, the importance of such a means of investigation is evident.

The apparatus in question consists of two concentric tubes so arranged as to give an exact indication of the translucency of the urine under examination after treatment with sulphuric acid.

As nothing more than a description of the instrument is given, the author not having as yet determined the proper amounts of urine and sulphuric acid to be used, it is impossible to judge whether he has attained the desired end.

The Respiratory Movements in Man. By Dr. ARTHUR RANSOME, M.A., M.B. —The author, having found that all attempts hitherto made, whether by means of stethometers or otherwise, to investigate the movements of the chest-walls in health and disease, had resulted in obtaining merely the resultant of these movements, was led to construct an instrument, but imperfectly described in the text and diagrams, "which records upon three dials, with considerable accuracy, the movements of a small rod when it is placed upon different points of the chest-wall." With this the degree of variation in the various motions can be shown graphically by means of lines drawn to a scale, as is done for other purposes. A series of diagrams, representing the chest movements in health and disease, accompanies the paper.

As in both men and women a large part of the respiratory act is, in ordinary breathing, performed by the diaphragm, no information can be obtained from the use of *any* stethometer, except in deep or forced respiration. When this last condition is complied with, the chief movements are forward and upward, the former the more equable of the two, and starting more rapidly at first than the upward motion. In men the upward movement takes place chiefly at the latter portion of the respiratory act.

"In most full-chested men the earliest portions of the expansive act seem to be accomplished by an increase of the action of the diaphragm, then, in an ascending order from below upwards, the ribs are gradually raised by their special muscles, and the lower ribs are probably at the same time straightened at the anterior angle between them and their costal cartilages; at last, for the final effort of inspiration, the head and shoulders are fixed, the spinal column is curved backwards, and all the muscles capable of producing upward movement of the bony cage are excited to their utmost power."

In expiration after this effort, the operation is reversed, the diaphragm gives way first, the abdomen, in spare subjects, collapses, and the ribs descend gradually, the upper ribs commencing the descent first. Many variations may, however, occur through the action of the will, and hence, perhaps, the want of correspondence remarkable between the accounts of different observers. Even in females who display the more truly "superior costal" type of breathing, the lower ribs have slightly the precedence, both in ordinary and extraordinary breathing. The actual extent of motion varies so much in different individuals and in different regions of the chest, that it is impossible to make an accurate average estimate of it, but from numerous observations it seems that the sternum, tied as it is by clavicles and ribs, and capable of moving but forward and upward, has a greater extent of motion than the clavicles; its lower portion moving more than the upper, though not so much as might be expected after observing the play of the lower ribs. This may be due to the hampering effect of the diaphragm and abdominal muscles. At the clavicles and upper two ribs there is but little outward movement; at the third rib, however, this motion is distinct.

The clavicles show more upward than forward movement, and are especially affected by the final effort of breathing. The ribs at their anterior ends move more decidedly upward than the sternum, and in all below the third their forward movement is more marked. With regard to the effect of position on healthy respiration, the author finds, that, when the hands are supported on a level with the head, as in the position ordinarily chosen by asthmatic patients during a paroxysm of dyspnœa, the respiratory movements are most free in all directions.

In women, though the breathing is chiefly costal, there is no increased power of raising the ribs in forced inspiration. As might be expected from the greater mobility of their frames, children possess a large relative power of motion of the chest-walls, while in old age this power is much reduced.

When increased mobility and size are found on either side, they are associated with, and probably due to, increased muscular development of that side. The greatest amount of movement of ribs and sternum was found in those of considerable muscular development. Disease is attended by great variations in these movements. There is probably always a diminution in mobility, which may be confined to one or extend to all the planes of motion. In phthisis probably one of the earliest symptoms of incipient deposit is diminution of movement in the affected side. Later, as softening occurs, there is often greater freedom of motion over the diseased lung. In many cases of disease the upward movement is exaggerated as compared with the forward, as if to compensate for the lung's loss of elasticity. In pleurisy, whilst the upward movement is little, if at all, diminished, the forward is reduced to the lowest possible point, and retained merely enough to allow of the rotation of the rib on its vertebral axis.

Numerous diagrams and tables attest the accuracy and painstaking character of the author's observations. H. B. H.

ART. XXII.—*Transactions of American State Medical Societies.*

1. *Transactions of the Medical Society of the State of Pennsylvania, at its Twenty-second Annual Session, June*, 1871. 8vo. pp. 384.
2. *Transactions of the Second Annual Session of the Medical Society of Virginia, October*, 1871. 8vo. pp. 116.
3. *Transactions of the Medical Society of the State of West Virginia, June*, 1871. 8vo. pp. 86.
4. *Proceedings of the Second Annual Session of the State Medical Association of Arkansas, November*, 1871. 8vo. pp. 40.

1. THE Twenty-second Annual Session of the *State Medical Society of Pennsylvania* was opened by an address from Dr. S. D. GROSS, President, which is replete with good, practical sense, in reference to medical subjects of a general character.

Following this is a "Report of Six Hundred Cases of Diseases of the Ear," by Dr. TURNBULL, of Philadelphia, in which we do not perceive that there is developed any new fact in relation to the pathology or treatment of any of the prominent ailments of the auditory apparatus.

Next is described a "Case of Thyroid Dislocation of the Hip Joint," by Dr. B. LEE, of Philadelphia, which occurred in the second stage of coxalgia, reduction being effected by manipulation, and restoring to the patient, a female child, the capability of walking erect, for a considerable distance, with the aid of a crutch and cane, and with comparatively little fatigue.

The next paper, by the same writer, describes "A Modification of Taylor's Splint for Posterior Curvature of the Spine."

The paper by Dr. T. H. SQUIRE, of Elmira, N. Y., on a "New Prostatic Catheter," has been already published in this Journal (No. for Oct. 1871, p. 393).

Next in order follow "*Reports*" from seventeen "*County Societies*." Several of these present interesting sketches of the medical topography, hydrography, and meteorology of the counties from which they emenate, with brief notices of the population, its character, condition, and occupations. Some of the reports embrace also the histories of cases of disease, surgical operations, and of anomalous, complicated, and instrumental cases of obstetrics.

The medical history of those portions of Pennsylvania embraced in the above reports, for the twelve months preceding June, 1871, is in the highest degree favourable. The statement of all the reports is that "*no wide-spread epidemic* has prevailed within our borders." Even the usual endemic diseases have not been met with, either in number or in severity, to the same extent as in former years.

It is a curious circumstance that throughout all those portions of the State that have been heard from, the several forms of periodical fevers are rapidly disappearing. Even in those localities described as the great laboratories of miasmata they are much less prevalent than they were formerly. With the decrease of intermitting and remitting fevers, there is everywhere, on the other hand, a very considerable increase in the prevalence of typhoid fever, generally of a mild character, although in some few localities it was attended with considerable mortality.

Diphtheria has continued to prevail throughout the State, very generally, ever since its occurrence as a wide-spread, devastating epidemic, some ten or twelve years ago. In but few cases, however, during 1870–71, did it assume any of its previous malignancy of character. The deaths caused by it were but

few. The true character of the disease has become better known to our physicians, and in consequence it has been subjected to a more rational and successful treatment. The fruitless attempt made, at first, to arrest the disease by the local application of caustic and various stimulating washes, has been pretty generally succeeded by the better plan of the application of cold to the throat externally, and to the fauces internally, by giving the patient, at intervals, powdered ice, to be gradually swallowed.

Of the eruptive fevers, those reported to have chiefly prevailed were scarlatina and rubeola. The former occurred generally in localities limited in extent, and was, for the most part, of a very mild character. In many of the occurrences of measles, the cases were complicated with affections of the chest and lungs. But even when this was the case, under judicious treatment, the disease was productive of a comparatively small mortality.

In the report from Fayette County, there is an account by Dr. PHILLIPS, of Connellsville, of a few cases "of a very malignant form of puerperal fever." The cases, which were few in number, were confined to a small neighbourhood, occupied almost exclusively by miners, living in small, crowded houses, situated in low, damp localities. Dr. P. was unable to detect any evidence of contagion; other women confined about the same time, within a few yards of those labouring under the fever, and attended by the same physician and nurses, were unaffected. There was prevailing, at the time, in the same community, a tendency to attacks of typhoid fever.

We would call especial attention to the interesting account, contained in the quite elaborate report from the Philadelphia County Medical Society, of the "Relapsing Fever," which prevailed in the county during the years 1870–71. Thousands (over five) of persons were attacked, with an estimated mortality of about 8.42 per cent.

In the same report will be found, also, an account of the *yellow fever* which prevailed in certain localities during the months of July, August, and September, 1870. The mortality amounted to *thirteen* out of seventeen attacked—nearly 77 per cent. Average duration of sickness in fatal cases, only *five* days.

2. The Second Annual Session of the *Medical Society of Virginia* was opened by an excellent address from the President, Dr. ROBERT S. PAYNE.

This is followed by a paper on "Dysmenorrhœa," with special reference to its treatment, surgical and constitutional, comprising the history of four cases in illustration of the views advanced by the author, Dr. JOHN H. CLAIBORNE.

The views set forth by Dr. C., in respect to the character and treatment of dysmenorrhœa, are evidently based upon a close clinical study of the disease. Dysmenorrhœa, Dr. C. very correctly remarks, is to be viewed only as a symptom of disease, and not as a disease of itself. The pain attendant upon "difficult menstruation," may be simply "neuralgic in its character; or, there may be congestion or inflammation of the uterus to account for it; or, it may be dependent on some toxæmic condition of blood, as expressed by the gouty or rheumatic diathesis; or, there may be some obstruction or stricture or flexion of the cervix uteri, diminishing its diameter, and thus damming up the menstrual flow, or preventing the discharge of some adventitious membrane developed from the lining tissue of the uterus; or, there may be a polypoid tumour within the organ, the pain in such case being but the simulation of the process and agony of labour. Each and all of these causes, of course, must be taken into consideration in the investigation of every case we meet with, if we would institute a rational and successful treatment. More than one cause may be present in the same case. We thus often find neuralgia, and engorgement of the womb

and ovaries, and the rheumatic diathesis, all combined to aggravate the sufferings of the dysmenorrhagic patient."

A report from the Committee on Epidemics of the "Tide Water District," is devoted to a consideration of "Paludal Fever," as it presents itself in the miasmatic portions of Virginia. The report is drawn up by Dr. ALFRED T. TEBAULT, and is full and satisfactory. It does not, however, contain anything adapted to throw much additional light upon pathology or therapeutics of periodical fevers.

The report from the "Committee on Epidemics of the Valley District," treats of "Typhoid Fever." It is drawn up by Dr. A. M. FAUNTLEROY. It is short and but little instructive. It is a curious fact, that prior to the appearance of typhoid fever in 1840-41, remittent and intermittent fevers were the prevailing febrile affections of the Valley district of Virginia. Subsequently to the change from the periodical type, in the prevailing fevers of the Valley region, it is remarked that the diseases of the same regions which before had manifested a frank sthenic phase, became decidedly asthenic, requiring a radical change in the remedial measures previously employed. "It is a notable fact," says Dr. F., "that, within the last few years, the periodical forms of febrile diseases are again becoming frequent. Well-defined cases of intermittent fever have come under my observation, occurring in subjects who had never been outside the Valley country."

A short paper, by Dr. F. HORNER, Jr., on the "*Cattle Disease*," has for its object to elicit attention to "the identity of the diseases of the lower animals with those of the human species." We fear, however, that they who seek for the facts and arguments in elucidation of the subject, will search in vain for them in the paper before us.

Another *voluntary contribution*, is the history of a "Case of Rheumatism," by Dr. W. B. GRAY. The peculiarity of the case, which occurred in a lad 12 years of age, is that the attack of acute articular rheumatism gave rise to inflammation, absorption, abscess, necrosis, and finally fracture of bone by muscular contraction.

There is a short paper in favour of the "Hypodermic Method of Treatment," by Dr. W. W. PARKER.

The volume closes with the report of a "Discussion on Inflammation." It will admit of no useful analysis.

3. The fourth annual meeting of the *State Medical Society of West Virginia*, was opened by an address from the President, Dr. W. J. BLAND, from which we quote the following brief sketch of the general topography of the State, and the prominent diseases prevalent within its bounds:—

"On the east the Alleghany Mountains constitute our boundary. They are covered with a dense growth of forest trees, and the country immediately along the range is very sparsely settled. On the west the Ohio river; on the north the Maryland and Pennsylvania State lines, and on the south the Kentucky State line, constitute our boundaries. The difference between the water-level at Glenville, in Gilmer County, and the mouth of the Great Kanawha River, a distance of one hundred miles, is only 337 feet; between Glenville and Weston, in Lewis County, a distance of twenty-seven miles, it is 277 feet; between Weston and Buchannon, in Upshur County, a distance of fifteen miles, nearly 500 feet; between Buchanan and Beverly, in Randolph County, a distance of thirty miles, it is 600 feet; and from Beverly to the summit of the Alleghanies, nearly 1700 feet.

"Along the Ohio River the country is densely populated, and in many of the counties intermittent fever prevails, while in the counties immediately back from the river, it is rarely met with, and as you approach the centre of the

State, I do not remember to have heard of a single original case. I believe there is no locality in the State that is entirely free from typhoid fever.

"Our winters usually commence about the middle of November, or first of December, and continue until the last of March or the first of April. May is often cold. It is with June our summer properly commences. July and August are generally sultry. In September the nights are cool, and the days pleasant. Our climate is very changeable, some days in summer being cool enough to render a fire almost a necessity. I have known heavy frosts to occur in the county in which I live (Lewis) in June, and have experienced days in January and February almost too warm for comfort. These extremes are unfavourable to health, and productive of pleurisies, pneumonias, rheumatisms, etc., often terminating in consumption.

"According to my recollection, scarlet fever and measles have made their appearance about every five years. The first of these diseases is often very fatal. They are both specific in their character, and, no doubt, contagious, and I am not aware that we have as yet a remedy for either. It is conceded, I believe, by almost every intelligent practitioner, that it is not in the power of science to cut short in its course, either scarlet fever, measles, smallpox, or typhoid fever. All that we can expect to accomplish is, by proper attention and therapeutic means, to obviate the secondary affections, which may and often do prove fatal to the lives of our patients."

"The subject of embolism and thrombosis has for the last few years attracted the attention of the profession. Dr. W. Coles, of Parkersburg, has written an elaborate paper on the subject, showing its relationship to other diseases, and demonstrating that it is, in very many, the immediate cause of death."

The first of the papers is on the "Use and Importance of the Microscope to the Medical Profession," by Dr. B. W. Allen, in which the author has presented a very fair exposition of the present state of opinion in respect to it.

The second article is on "Sanitary Science; its Study, Cultivation and Present State," by Dr. Richard Blum. To the consideration of this important subject the author has devoted only six medium octavo pages. Nothwithstanding the unsatisfactory, loose, and imperfect manner in which the subjects of this paper are discussed, its general positions, so far as they go, are correct and pertinent, however practically valueless.

The third paper, on "Anæsthetics," by Dr. Walter Coles, presents a tolerably fair outline of the present state of medical opinion in respect to the safety and value of anæsthetics in surgery and obstetrics generally, and of the relative value and safety of the leading anæsthetics now in use. By combining the statistics furnished by Dr. E. Andrews, of Chicago, Illinois, with those of Dr. Richardson, of London, England, we obtain the following general view of the absolute and relative mortality caused by the several anæsthetic agents, of which Dr. C. treats in the paper before us, in a given number of carefully reported cases.

Ether	4	deaths	to	92,815	inhalations	=1	to 23,204
Chloroform	53	"		152,260	"	=1	to 2,873
Mixture Chloroform and Ether	2	"		11,176	"	=1	to 5,588
Bichloride of Methylene . .	2	"		10,000	"	=1	to 5,000

The above figures the author regards as "the most valuable and reliable that have ever been published in reference to the mortality in anæsthesia. They are, perhaps, above rather than under the mark, and demonstrate a state of facts so absolutely at variance with the received opinions of five years ago, as to become perfectly startling. They indicate that chloroform is eight times more dangerous than ether; twice as dangerous as an equal mixture of chloroform and ether, and, as far as experience goes, it is more fatal that bichloride of methylene. In view of these facts, the question arises, '*Will chloroform maintain its present popularity as an anæsthetic in surgery?*' We do not believe it will; unless some method, other than we have at present, is devised to

lessen the risk attendant upon its use, we cannot but think that its popularity must decline. In the face of such figures as we have adduced, chloroform cannot and ought not to supersede ether in hospital practice. The inconveniences attending the use of ether are more than compensated for in the risks from chloroform."

Next in order is a paper on the "Temperature of the Body in Health and Disease, as measured by the Thermometer," by Dr. W. H. SHARP. The study of the temperature of the human body in health at the different periods of life and during disease, its variations in the different forms of disease and at the several stages of each, and, from the various changes found to take place, to deduce the true bearing, if any they have, in reference to the nature and seat of disease, and its favourable or unfavourable course, is a department of pathological diagnosis and prognosis which has only recently attracted the attention of physicians. A general but a very interesting outline of the present condition of the knowledge of the profession in respect to the subject, and of the true estimate of its value when applied to the clinical study of disease, will be found in the paper of Dr. S. To give our readers any satisfactory analysis of the information embraced in the paper would not be possible without extending our notice of it to an objectionable length.

The sixth article is a "Report from the Committee on the Medical Botany of the State of West Virginia," prepared by Dr. A. S. TODD. The author furnishes us with a list of some sixty indigenous plants, with a short notice of their imputed properties as therapeutic agents. Most of them will be found to be included in the lists of indigenous remedies given in our National Dispensatory, and are in daily use by the physicians of most parts of the United States.

The next paper is a "Report on the Topography, Climatology, and Epidemic Diseases of Upshur County, W. V., by Dr. E. S. BRONSON.

A brief but interesting history is presented by Dr. H. I. WIESEL, of five cases of "Trichinosis," occurring in the same family soon after partaking at supper of "raw smoked ham," which, upon microscopic examination, was found to be swarming with *trichinæ*. One of these individuals was a male, 28 years old. After suffering from the presence of trichinæ four consecutive months, he became gradually restored to "all his pristine health and vigour."

Another of the patients was a female, 22 years old. She remained without sleep for the space of 30 days. She made a very rapid recovery, completely regaining her health by the thirty-sixth day from the commencement of her attack. She was then in the third month of pregnancy. Two or three times every day, up to the period of the child's birth, at full time, she experienced attacks of fainting. After delivery, which was perfectly natural and easy, both she and her child remained in perfect health.

Another of the party, a female infant two and a half years old, daughter of the latter, ate a very small piece of the ham. On the forty-fifth day she began to improve, and was, at the date of Dr. W.'s paper (June 7, 1871), "a healthy, robust child."

A female ate a small piece of ham, on the fourth day after she felt pain in all her limbs, and in the bowels. Had diarrhœa, with tenesmus. Took purgatives. Was well about the twelfth day.

Another female, on the fourth day after partaking of the ham, felt pain in the arms and legs. In the latter the pain continued until the fortieth day. At first there was inability to extend the legs. Took purgatives from the first day of the attack, and was soon relieved of her abdominal pain and diarrhœa.

The remaining two papers are devoted each to the description of a new in-

strument. One for "the treatment of fracture of the lower jaw," by Dr. P. B. DAVIS; and the other of an "improved tonsillotome," by the same gentleman. The description of either instrument would scarcely convey a correct idea of its construction, without the drawings by which the original papers are accompanied.

4. The communications read during the session of the *State Medical Society of Arkansas* were:—

1st. "A Sanitary Survey of the City of Little Rock," by Dr. R. G. JENNINGS, which, though very short and superficial, leads to the general conclusion "that Little Rock is rightly to be considered a decidedly healthy city."

2d. "Knee-Joint Amputations," with the report of two cases, by Dr. W. W. HAWKINS. "In the two cases reported, the antero-posterior flap, with retention of the patella, was adopted, though the condyles were shaved off." The latter procedure, however, Dr. H. considers unnecessary, and in future operations has decided to follow the method pursued by Dr. STEPHEN SMITH (*American Journal of the Medical Sciences*, 1870); which, Dr. H. remarks, certainly admits of a more thorough drainage, and places the cicatrix in a position less liable to be pressed upon by an artificial limb—two circumstances not to be disregarded.

3d. "Case of Malarial Hæmaturia," by Dr. E. R. DU VAL. The most prominent symptoms in this case were intense nausea, bloody urine, and excessive prostration. The energies of both body and mind "seemed in bonds to an influence, lion-like in power." The whole system appeared to have nearly attained the point of saturation with malarial poison—its poisonous action upon the ganglionic system doubtless was the cause of such varied and rare morbid manifestations. The clear indication was to support the system and to give quinia freely. The first it was difficult to accomplish, because of the gastric irritability, the latter was carried out as far as practicable by injections into the rectum.

It would appear from the current medical literature that the form of disease under consideration has recently prevailed in Texas, Louisiana, and Mississippi, occasionally with much fatality.

The volume closes with a short paper entitled "Climatology; Remarks on the Climatology of the United States," by Dr. GEO. W. LAWRENCE. It merely points out the vast importance of a complete series of observations, illustrative of the climatology of all portions of the country, and the best means of collecting, preserving, and collating such facts.

D. F. C.

ART. XXIII.—*Pulmonary Consumption; its Nature, Varieties, and Treatment. With an Analysis of One Thousand Cases to exemplify its Duration.* By C. J. B. WILLIAMS, M.D., F.R.S., Senior Consulting Physician to the Hospital for Consumption, Brompton; and CHARLES THEODORE WILLIAMS, M.A., M.D., Oxon., Physician to the Hospital for Consumption, Brompton. 8vo. pp. 315. Philadelphia: Henry C. Lea, 1872.

THE elder Dr. Williams is well known in this country as the author of a work on the "Pathology and Diagnosis of Diseases of the Chest," and of numerous contributions on the same subject to periodical literature. During a period of nearly fifty years, he says, the nature of pulmonary consumption has been the

object of constant study. In early life he enjoyed the advantages of the teachings of such men as Alison, Laennec, Andral, and Chomel, and he has since had numerous opportunities, in both hospital and private practice, of observing the disease and of studying its post-mortem appearances. In the preparation of the present work he has been assisted by his son, Dr. C. T. Williams, who contributes chapters on "Family Predisposition and Certain other Causes of Consumption," "Hæmoptysis and the Hæmorrhagic Variety of Consumption," "The Duration of Pulmonary Consumption," and some of the chapters on the treatment of the disease.

Dr. Williams, although a pupil of Laennec, soon perceived that his view of the nature of consumption was too exclusive. It appeared to him much more probable that it arises—no matter under what form—"from a decline or deficiency of vitality in the natural bioplasm or germinal matter, and this deficiency manifests its effects not only in a general wasting or atrophy of the whole body, but in a peculiar degradation, chiefly in the lungs and lymphatic system, of portions of this bioplasm into a sluggish, low-lived, yet proliferating matter, which, instead of maintaining the nutrition and integrity of the tissues, clogs them and irritates them with a substance which is more or less prone to decay, and eventually involves them also in its own disintegration and destruction." This degraded bioplasm, which he calls phthinoplasm, may be thrown out locally, as a result of inflammation, or it may arise more spontaneously in divers points of the bioplasm in its ordinary receptacles, the lymphatic glandular system, and then it commonly appears in the form of miliary tubercles, scattered through the adenoid tissue of the lungs. It will appear from the above that Dr. Williams does not look upon phthisis and tuberculosis—words which he, nevertheless, constantly uses rather loosely—as identical conditions, but the distinction between them is not so distinctly pointed out by him as has been done recently by some of the German pathologists. While admitting, as we have already said he does, the inflammatory origin of many cases of the former disease, he thinks that in others it arises from a degraded modification of the common nutrition of the tissues of the lungs, entirely independent of inflammation.

The true tubercle he traces to a hypertrophy of a glandular tissue which he believes to be extensively distributed throughout the lungs, and which he calls adenoid tissue. This tissue is occasionally excited to increased growth, and in this way performs an essential part in the formation of miliary tubercle. The predisposing and exciting causes of this hypertrophy are not so clearly stated as we could wish, and appear to be various. Dr. Williams goes beyond Niemeyer and his followers in asserting that these miliary tubercles are sometimes themselves the direct results of inflammation. In the greater number of instances, however, he refers them to an altered condition of the blood, and hence their occasional occurrence after scarlet fever, and some of the other acute exanthemata. More frequently, however, this alteration of the blood is brought about by chronic disease, as by phthisis, scrofula, and prolonged suppuration. He does not apparently agree with Oppolzer, who believes that this effect is produced by the absorption of corpuscular bodies, which, when they are taken into the blood, give rise to an extravasation of white blood-corpuscles in the lungs or in some of the other organs of the body, which, with an outgrowth of connective tissue, form, in the opinion of the latter, miliary tubercles. Under the influence of depressing causes, such as deficiency of light and food, a vitiated atmosphere, depressing emotions, and the like, the external glands of the body may undergo enlargement, and from a similar cause these minute glandular bodies may become hypertrophied. This theory of the origin of tubercles certainly has the advantage that it will explain all cases of tuberculosis, while that of

Niemeyer fails in those rare cases in which no previous caseous degeneration of the lungs or glands, or any disease which he has particularized as likely to be followed by tuberculosis, can be found In both cases, moreover, there is "an excessive multiplication of perishable cells doomed to speedy decay." These cells, he says, have the closest resemblance to leucocytes, and there are good grounds for believing that they are identical not only with them, but with the corpuscles of fibrinous exudations and with pus-cells.

In regard to the occurrence of consumption after the cessation of habitual discharges, Dr. Williams says: "The sudden healing of old ulcers and of the suppurating wounds of setons and issues is often quoted by authors as a cause of pulmonary consumption; but it is not clear that the cessation of the discharge may not be due to the morbid change in the lungs having already begun. Compared with the artificial production of tubercles in animals, which we shall hereafter notice, it appears as likely that the presence of pus in any part, whether by production or inoculation, may have a deteriorating influence on the sarcophytes (leucocytes) of the blood and lymphatics, as that the cessation of suppuration should have a similar effect. But it is quite rational—and perhaps the safest view—to admit both influences in different cases."

The statistical part of the work, as we have already said, was confided by the author to his son, and is based upon one thousand cases. The space at our command will only permit us to give a few of the results of Dr. Williams's experiences. They are as follows:—

Phthisis was preceded by pleurisy and pleuro-pneumonia in 149 cases; by bronchitis in 118; by scrofulous abscesses in 12, and by hooping-cough in 6. The number arising from pleuro-pneumonia and bronchitis is very large, reaching a total of 267, or more than one-quarter of the whole, and deserves attention as showing statistically the influence of these diseases as direct sources of consumption. Of the 1000 patients, 198 are ascertained to have died, and the greater part of these succumbed to the gradual waste and decay of phthisis; 15 died of phthisical complication.

8	patients lived	1 year	and under	2,			
22	" "	2 years	"	3,			
18	" "	3 "	"	4,			
23	" "	4 "	"	5,			
24	" "	5 "	to	9	inclusive,		
31	" "	10 "	"	14	"		
12	" "	15 "	"	19	"		
9	" "	20 "	"	30	"		

Average duration of disease in 119 males (dead) 8 years 4.72 months.
" " " 70 females (dead) 6 years 8.67 months.

The results of the influence of age and sex on the duration of consumption may be thus summed up:—

1stly. The duration is longer in proportion as the age of the attack is later, the retarding influence of age being more conspicuous among males than among females.

2dly. Among the females the time of attack is, on the average, earlier than among males.

3dly. The duration o the disease is shorter.

4thly. The age reached by consumptive females is less.

In 12 per cent. of the one thousand patients a direct hereditary predisposition could be traced, and in 48 per cent. a family predisposition. Family predisposition was found to be much more common among women than among men.

The transmission of phthisis is more common through the mother than through the father. Fathers transmit more frequently to sons, and mothers to daughters, than the converse. A family predisposition to phthisis hurries the onset of the disease and thus shortens the duration of life. Of the 1000 patients, 625 were males and 375 were females. The average age and time of attack for males were 29.47, for females 26.06.

In regard to the relation which hæmoptysis bears to phthisis, Dr. C. T. Williams says, that he has generally been able to detect signs of disease during life in the lungs of all those patients who have had extensive hæmoptysis, in connection with heart disease, injury to the chest, or disorder of the menstruation. It is very probable that this may be true, as a rule, but it is inconceivable that hemorrhage may not occur in a few instances simply from want of nutrition of the walls of the bloodvessels, where no disease of the lungs can be detected by the physical method of diagnosis. He refers to one of Niemeyer's cases, in which hemorrhage was supposed to have induced the disease, and in which the physical signs gave evidence of disease of the apex of one lung. This, he says, is surely a strong argument against a hemorrhagic origin for the disease; for blood effused from the bronchial mucous membrane must obey the laws of gravity, and flow towards the lower portions of the lungs. This does not seem to us necessarily true. If the hemorrhage be large in amount and come from the apex of the lung, most of the blood will be expectorated, but a little may flow into the air-cells and remain there exciting inflammation when decomposition takes place. The same thing may happen when there is only a slight oozing of blood. It is difficult to understand why blood may not remain under these circumstances at the upper part of the lung as well as mucous or softened caseous matter. In regard to the setting up of inflammation by extravasated blood, the writer of this notice would say that he has often noticed an increase of temperature in patients a few days after the occurrence of a hemorrhage, which could be attributed to no other cause than this.

Several chapters are almost wholly taken up with the reports of cases illustrating the different forms of consumption, and these, we have no doubt, will be regarded by many as among the most valuable in the book. We commend those chapters of the book in which the treatment of phthisis is discussed to the attention of those physicians who think that the adoption of the theory that inflammation is sometimes the exciting cause of phthisis, must of necessity lead to errors of practice. Dr. Williams seems to have held, for the past fifty years, the same views of the nature of the disease as he now holds, and to have prescribed in accordance with these views, and yet he is able to say, "When I state that the average duration of life of phthisis has, during my experience of forty years, been at least quadrupled, or raised from two to eight years, I say what is below the actual results, as calculated by my son; for, of the 1000 cases, 802 were still living at the last report, and many of them are likely to live for years to come." The formulæ which Dr. Williams has found most useful in the management of consumption and its complications are introduced into this part of the book. They have been useful in his hands, and others will no doubt find them adapted for the conditions in which he recommends them.

The last chapter is on the subject of climate, and is by Dr. C. T. Williams.

Our notice of Dr. Williams' book cannot be better brought to a close than by an expression of the pleasure and profit we have ourselves derived from reading it, and under these circumstances we have no hesitation in recommending it most cordially to the profession. J. H. H.

ART. XXIV.—*On the Treatment of Pulmonary Consumption by Hygiene, Climate, and Medicine, in its Connection with Modern Doctrines.* By J. HENRY BENNET, M.D., M.R.C.P. Second edition. 8vo. pp. 190. London: J. & A. Churchill, 1872.

The same. New York: D. Appleton & Co., 1871.

THE first edition of this book, which was published in 1866, and briefly noticed in the October number of this Journal for 1868, was a purely clinical treatise. In the second edition the author has, however, thought proper to extend the scope of his work by adding a chapter "On the Intimate Nature and Origin of Tubercle." He has also introduced the reports of several cases illustrative of the cure and arrest of pulmonary phthisis, and devotes several pages to answering the questions, "What are cured consumptives to do in life? Can they marry?"

The author is evidently a great admirer of Professor Bennett, of Edinburgh, whom he calls his namesake, and whose views on the pathology of phthisis he considers much more correct than those professed by the German pathologists. Inasmuch as his quotations from the latter are all second-hand, we may be pardoned for saying that his want of knowledge of the language is the reason of the very partial knowledge he has of them. Otherwise he would scarcely say that tubercle was regarded by all German pathologists as a growth of connective tissue. Niemeyer, Oppolzer, and Waldenburg, while making a distinction between tuberculosis and phthisis, which we think is clinically as well as pathologically demonstrable, and believing that the latter is generally the result of a previous inflammation, all state that miliary tubercles are depositions from the blood; the last, and we believe at least one of the others, holding that they are largely made up of extravasated white blood-corpuscles, which may or may not excite a growth of connective tissue in their immediate vicinity. Dr. Bennet is, moreover, afraid that the general adoption of the theory of the inflammatory origin of phthisis will induce many physicians to have recourse to the use of antiphlogistics in its treatment. Although it can scarcely be urged as an objection to a well-founded theory, that it may lead to occasional errors of practice, we do not see how this is likely to arise from the adoption of the theory in question. Is Dr. Bennet not aware that it is no longer the fashion to treat even frank inflammations by antiphlogistic remedies, and that *à fortiori* the restorative treatment would be prescribed in an inflammation which is confessedly of low grade? Niemeyer, it is true, advises occasionally the local abstraction of a small amount of blood, but he also recommends, among other things, rest, and the pill to which his name is generally given, composed of quinia, digitalis, and opium, which can scarcely be looked upon as debilitating in its therapeutic action. Our author does not seem to have noticed in the passage which he quotes from the article on Pulmonary Consumption in Reynolds's *System of Medicine*, that Professor Bennett there states that when an individual is predisposed to phthisis, "any accidental irritation of the lungs, often inappreciable and undetectable, causes a limited congestion here and there in the pulmonary organs, which terminates in more or less exudation of the liquor sanguinis. This exudation coagulating causes the miliary and infiltrated forms of tubercle previously described, which, partaking of the diminished vital power of the organism, instead of being transformed into the pus characteristic of a similar exudation in a healthy person, produces the small, irregular, and imperfect bodies called tubercle corpuscles. Instead of

cells which are rapidly produced, broken down, and absorbed, as in pneumonia, we have numerous molecules and bodies resembling ill-formed nuclei. In short, we have a chronic exudation in which the vitality is so lowered that it tends to disintegration, and to produce the lowest kinds of organic forms, *i. e.*, molecular granules and nuclei. It seems to us that the only material difference between Niemeyer's views and Professor Bennett's is that the latter does not make a distinction between tuberculosis and phthisis.

Dr. Bennet is again wrong in saying that the German school of pathologists deny the influence of a hereditary or acquired predisposition to phthisis. This is most distinctly admitted by Niemeyer and Oppolzer, for the *Vulnerabilitat* of the former can hardly be better translated than by the word predisposition, and the testimony of the latter on this point is given in an even more unqualified manner. The constitutional condition which attends phthisis is therefore as distinctly admitted by these and other distinguished observers as by Professor Bennett himself, to consist in a depression of the function of nutrition.

Although believing that a certain number of cases of phthisis are capable of cure, Dr. Bennet expresses the opinion that it is decidedly more prudent, more judicious, even for thoroughly cured consumptives, not to return to active life, unless socially or morally obliged to do so. Of course, in cases in which the disease is merely arrested, it is even more important that the patients should abandon active and ambitious careers. In regard to the question of marriage, he says that the dangers of matrimony for consumptives differ according to sex. In men there is often an absence of proper discretion, and consequently marriage is an additional cause of debility and exhaustion. In women the danger is different; marriage does not with them try or fatigue the constitution to any great extent, unless followed by uterine disease, or by pregnancy, the latter of which is very apt to occur, inasmuch as five women out of six are fertile. Thus, the consumptive will most likely have to encounter pregnancy, confinement, and nursing, with all the attendant shocks and drains on the vitality. As a result, pulmonary phthisis is constantly accelerated and rendered fatal. A woman with a predisposition to consumption is therefore more likely to have it developed into actual disease than a man with a similar tendency. A man, the author thinks, once he has emerged from the phthisical diathesis, may marry and have children who may be strong and live. But he should be careful to choose a young and healthy wife, of a healthy stock, born and bred in the country; he must be discreet in married life, and he must bring up his children hygienically, in the country, devoting them to country pursuits.

The part of the book which still remains to be noticed is devoted to the discussion of the treatment of consumption by hygiene, climate, and medicine, and of the author's views on these points we have no criticism to make. Dr. Bennet has had unusual advantages in the study of the disease, having himself been a sufferer from it, and certainly adopted in his own case a course of treatment which has been attended with most favourable results. We therefore commend this part of the book to our readers as containing the advice of a man who is entitled to speak authoritatively.

J. H. H.

ART. XXV.—*On the Treatment of Fractures of the Limbs.* By SAMPSON GAMGEE, F.R.S.E., etc., 8vo. pp. xvi., 296. London: J. & A. Churchill, 1871.

MR. GAMGEE, who as a writer is already favourably known to American surgeons, adopts, as a motto for his volume, the familiar lines of Horace, which

prescribe that the date of publication should be postponed until nine years after that of authorship, and begins his preface by quoting, from Mr. Trelawny's not very creditable volume of "Recollections," an anecdote of Lord Byron, who, it appears, being invited to put his thoughts on paper, declined to do so until he had had an opportunity to "chew the cud" previously.

We quite agree with Mr. Gamgee that authors of surgical works should think long and carefully before they write, and should duly consider what they have written before they print; but if they follow the plan which he avers he has himself pursued, of not publishing their thoughts till these have become old, they must not be disappointed if their works strike the reader as being less fresh and attractive than they would have been had they not undergone the drying process so thoroughly.

Mr. Gamgee's volume is an elaborate, earnest, and, upon the whole, able argument in advocacy of the universal employment of immovable apparatus in the treatment of fractures; and he evidently believes what he repeatedly asserts by implication if not directly, that it is merely the innate and invincible obstinacy and perverseness of surgeons in general, and their weak and blind reverence for authority, that prevents them from all seeing with his eyes and at once adopting his opinions.

We have been not a little amused at the account which Mr. Gamgee ingeniously gives of an interview which he had with the illustrious Malgaigne, and in which he undertook to show the French professor the error of his ways: "I took occasion," he says, "of a visit to St. Louis, to discuss with the very learned commentator of Ambroise Paré, some of the questions in the treatment of fracture, which had been the theme of controversy in his great works and in my memoirs. I flattered myself that I had successfully answered Monsieur Malgaigne's objections, on the basis of fact, when he convinced me of the hopelessness of the endeavour, by remarking—'Alors, monsieur, les faits chez vous ne sont pas tels que les faits chez nous.' It had not previously occurred to me," adds honest Mr. Gamgee, "that English and French surgical facts were different." Is it not sufficiently evident that "the very learned commentator of Ambroise Paré" meant not to say that English and French facts were different, but politely to insinuate that the "facts" with which his opinions had been opposed were no facts at all?

There is, to our mind, a great deal that is sound in Mr. Gamgee's teaching, and, at the risk of being set down as a blind follower of authority, we must add, some little error. We quite agree with the author that in almost every case reduction of a fracture should be practised as soon as possible, and that, in the case of the long bones, the neighbouring joints should be fixed by the splint or other apparatus employed; but we most emphatically dissent from the doctrine that circular compression of the limb at the seat of fracture is either necessary or desirable; on the contrary, we believe this to be one of the most mischievous and reprehensible practices that has ever been introduced in the treatment of these injuries.

The practical part of Mr. Gamgee's volume gives a clear and satisfactory description of the author's mode of applying immovable dressings, but really contains nothing that is new or that is not already familiar to all reading surgeons. Mr. Gamgee, in speaking of the starched bandage, very properly remarks that the bandage is in itself a comparatively unimportant part of the apparatus, the even and careful padding of the limb, and the application of pasteboard or other suitable splints, being the most essential parts of his mode of treatment.

We have no particular antipathy to immovable dressings in themselves, and

frequently employ them, in some form or other, in the later stages of fractures, when the repair of the injury is tolerably well advanced; but for recent cases they seem to us in no respect better, and in some respects not so good as other forms of apparatus. Nor are we at all dismayed by Mr. Gamgee's array of "facts," for equally convincing "facts" can be and have been adduced in favour of every conceivable mode of treatment. Take, for instance, the case of a fractured thigh. Mr. Gamgee tells us of one which he cured without shortening, there being "no difference whatever in the shape or length of the two lower limbs;" but do we not all know that equally good results have been claimed from the use of Smith's anterior splint, of the weight extension apparatus, and probably of every other instrument ever devised for the treatment of these cases? In what respect are Mr. Gamgee's facts better than those of other surgeons? For our own part, so sceptical are we as to so-called facts, that we gravely doubt whether any fractured thigh was ever cured without shortening by any method whatever; but should we accept Mr. Gamgee's facts we certainly could not reject the facts of other surgeons equally skilful and trustworthy.

Here, it seems to us, is a weak point of Mr. Gamgee's volume; the few (and he confesses that there are very few) who agree with him, are independent thinkers, and arrive at just conclusions; but all others are victims of "the paralyzing influence of routine," or of "the disturbing effect of plausible innovations," and either "prefer to have their thinking work done for them, and find it most congenial to their taste to move along in obedience to traditional impulse on beaten ways," or from "servile obedience to authority," fly to the other extreme, "once the yoke is broken," and illustrate the aphorism that "credulity and infidelity are extremes that often spring from and revert to a common source." Now, what we ask of Mr. Gamgee is that he should "put himself in our place," and say plainly whether we and others can reasonably be expected to adopt his convictions—because they are his—instead of our own; and whether he really thinks that the fact that others differ from him in opinion necessarily proves them to be either ignorant or incompetent. That Mr. Gamgee is intelligent we know from the excellent surgical work which he has done, and that he is perfectly sincere and honest is evident from every page of the volume before us; but we insist that the sole test of intelligence and sincerity in others shall not be a willingness to adopt his views.

Looking at Mr. Gamgee's book (which, by the way, is printed with extreme inaccuracy), as a whole, we are forced to believe that it will not much advance his reputation as a surgical writer. His pathological views—as to the process of repair in fractured bones, for instance—are not very modern, and have evidently not been revised since 1862, when he tells us his lectures were first delivered; while the clinical observations which he records fail, as we have already mentioned, to bring to our minds that conviction of the superiority of his mode of treatment over that of others, which is manifestly expected. We are not even convinced by the parenthetical remarks in Chapter VI. on the subject of digital compression of arteries, that this is an important remedy for surgical inflammations, though we believe it to be a valuable resource in certain cases of aneurism. Besides, we are constantly shocked by the very laudatory terms in which the author speaks of himself and his own devices, and the very contemptuous references which he makes to other surgeons, living or deceased, who are in no degree his inferiors. We will only instance his remarks upon Prof. Lister's mode of treating compound fractures, and his criticism upon his old antagonist, M. Malgaigne, which is really worthy of being quoted. "Full of learning but deficient in judgment, a keen critic, but an awkward manipu-

lator, Malgaigne exercised an influence on the surgery of France, from which it has scarcely recovered. His commentary on the works of Ambroise Paré is a monument of erudition, but"—mark the sequel—"his Treatise on Fractures and Dislocations is a singular specimen of poor surgery." The last volume of the *Traité des Fractures et des Luxations*, was published seventeen years ago; will Mr. Gamgee allow us to express the hope that his volume will in 1889 occupy as high a position in surgical literature as that "singular specimen of poor surgery" now does in all portions of the civilized world? J. A., JR.

ART. XXVI.—*Neuralgia and the Diseases that resemble it.* By FRANCIS E. ANSTIE, M.D. (Lond.), F.R.C.P., Senior Assistant Physician to Westminster Hospital, etc. 8vo. pp. viii., 296. London and New York: Macmillan & Co., 1871.

The same. New York: D. Appleton & Co., 1871.

IN this book Dr. Anstie claims for neuralgia a more independent position than it sometimes occupies, as a sort of appendage or symptom of various diatheses or diseases, and also puts forward a definite theory as to its nature and seat. The chapters in which these subjects are treated the author considers the most important in the book.

It will be found, however, that the work is as far from mere empty theorizing, on the one hand, as it is, on the other, from the self-styled practicality which affects to despise all carefully elaborated theories, merely because it dignifies its own crude guesses with the name of facts.

The first chapter gives the clinical history of neuralgia. The author considers the painful points of Valleix to be secondary symptoms developed in old neuralgias. Migraine, or sickheadache, is the youthful form of a more characteristic trigeminal neuralgia of later years. Angina pectoris is, on account of its importance, and its forming as it were a disease by itself, more completely treated in this chapter than the other neuralgias. The most efficient constitutional treatment is said to be the phosphate of iron, strychnia and quinia, and especially arsenic. For the paroxysm, ether and nitrite of amyl are recommended.

Under the head of complications are briefly described the nutritive changes of the skin and its appendages, the periosteum and the eye, the perverted secretions and the lesions of motility and ordinary sensation.

In Chapter III. Dr. Anstie first shows how frequently neuralgia is connected with other neuroses in the same patient, or occurs in members of neurotic families. Then, dwelling upon the character of the complications, the manner in which neuralgias affect groups of nerves having near anatomical connections with each other, the anatomical changes connected with the neuralgic pains of locomotor ataxy, and many physiological facts and theories, he concludes (in the book the conclusion comes first), that "the essential seat of every true neuralgia is the posterior root of the spinal nerve in which the pain is felt, and that the essential condition of the tissue of that nerve root is atrophy, which is usually non-inflammatory in origin."

The chapter upon treatment contains no startling announcements of wonderful new remedies, but a clear and reasonable statement of the advantages to be derived from the different agencies employed, among which hypodermic morphia, atropia, strychnia, iron, good, and especially fatty, food, and last but not

least, the constant galvanic current, hold the chief places. Iodide of potassium and quinia may be used in special cases. Zinc and phosphorus do not seem to be highly esteemed. The remarks upon religious and intellectual training, as well as those upon the same subject in the chapter upon etiology, are of the highest interest and importance, and would prove, perhaps, quite as instructive to the clergyman or teacher, as to the physician.

The second part treats of the differential diagnosis of diseases resembling neuralgia, namely, myalgia, spinal irritation, pains of hypochondriasis, locomotor ataxy, cerebral abscess, alcoholism, syphilis, rheumatism, gout, peripheral irritation, including colic, and dyspeptic headache. It may be seen from the number of important subjects suggested in this sketch, that the book is one which, from the condensation of its style and the absence of "padding," ill bears an attempt to compress its leading ideas into small compass, and we therefore can sincerely advise its thoughtful perusal both by the theorist and the practitioner. R. T. E.

ART. XXVII.—*The Skim-milk Treatment of Diabetes and Bright's Disease, with Clinical Observations on the Symptoms and Pathology of these Affections.* By ARTHUR SCOTT DONKIN, M.D., Edin., Lecturer on Medical Jurisprudence and Toxicology in the University of Durham. 12mo. pp. 317. London: Longmans, Green & Co., 1871.

DR. DONKIN is known in this country as the author of a series of papers in the *Lancet* on the milk treatment of various diseased conditions, especially of diabetes and of Bright's disease, and the interest which these have excited had induced him to publish the present volume. He has not, however, restricted himself to reproduce his original contributions in their previous form and scope, but has written an entirely new essay on a broader basis.

A full history of the milk treatment of disease is given in the first chapter; the physical, chemical, and therapeutic properties of milk, and its relation to the process of nutrition, being detailed in the next two chapters. The symptoms and pathology of diabetes are next described, and discussed; and, finally, the various plans of treatment of the disease which have been hitherto in use are subjected to a critical review by the author, who then gives his reasons for preferring the skim-milk treatment to all others. The remaining portion of the book is devoted to the consideration of the pathology, symptoms, and treatment of Bright's disease of the kidneys. We shall, however, notice only those chapters in which the treatment of these diseases by skim-milk is proposed. The other chapters contain little that is not generally known by well-informed members of our profession. We may say, however, in passing, that Dr. Donkin thinks that fatty substances, when taken as food, have a tendency in the diabetic subject, especially if the disease be far advanced, to become converted into glucose, and should therefore never form a part of his diet.

It occurred to Dr. Donkin that if milk did produce an injurious effect in diabetes, or if it failed to act beneficially, this might possibly be dependent on the presence of the large quantity of fatty matter it contains. The objection is usually made to its use in the treatment of this disease, that it contains from four to six per cent. of sugar of milk, or lactin, which is supposed to be readily converted into diabetic sugar in the system; but lactin, when it is administered as a constituent of milk, is converted into lactic acid (the casein of the milk

probably acting as a ferment), which is incapable of being converted into glucose. Skim-milk contains, therefore, not only casein, and solutions of various salts, but also a saccharine alimentary principle, capable of being assimilated in spite of the disease, while only a very small percentage of fatty matter remains if the cream has been thoroughly separated from the milk. Generally this may be done by allowing the milk to stand for some hours, but in some cases it is necessary to pass it through a filter. When milk is thus administered, it is said, in cases of diabetes, to remove the sugar from the urine, and to cure the disease, even when the meat regimen has completely failed after a protracted trial to accomplish this result.

The form of Bright's disease in which Dr. Donkin has found the skim-milk treatment to be most frequently followed by success is fatty degeneration of the kidney; in fact, he does not recommend it at all in the other two forms of chronic disease of these organs. In this disease, besides furnishing to the patient a highly nutrient and easily assimilated food, the skim-milk treatment has other advantages. Prominent among these is the diuresis produced by it, which of course relieves the renal tubules of the accumulation of fatty cells, and permits the kidneys to resume their proper functions.

In regard to the mode of administering the skim-milk, Dr. Donkin says it should constitute, when the treatment is fully instituted, the sole article of food, until the sugar in the one case, and the albumen in the other, has disappeared from the urine; and it is not to be given in large quantities at a time, but in small and carefully measured quantities, and at regular intervals. "During the first day of the treatment, half a teacupful of skim-milk may be given every two or three hours, and on the second day, double the quantity at the same intervals; on the third day, half a pint may be allowed for each dose, and the intervals increased to three or four hours, so that, in all, three pints are consumed; on the fourth day, four pints may be given; on the fifth or sixth, five pints; and should this augmented quantity produce no inconvenience, and the patient's appetite be good, as it generally is under the treatment, the quantity may be raised to six or seven pints daily, but after this no further increase should be permitted, except in certain cases in patients of large frames and keen appetites, to whom eight or even nine pints may be allowed."

The histories of several cases of diabetes and of several cases of fatty degeneration of the kidneys are given. In the former set of cases, the effect of treatment is said to have been almost magical, twenty-four hours being generally sufficient for the production of a marked improvement. The feeling of debility and the emaciation which are such common attendants upon the disease, soon disappear; the urine diminishes in quantity, and will be found to contain less sugar; the patient ceases to suffer from thirst, voracious appetite, and dryness of the skin, and, in cases in which the disease has not been of very long continuance, a permanent cure is said to have been effected.

In Bright's disease the results obtained are claimed to have been scarcely less remarkable, the treatment having caused the disappearance of albumen from the urine in many cases which had been unsuccessfully treated by other remedies, and in other and incurable cases having produced great relief.

J. H. H.

ART. XXVIII.—*A Practical Treatise on Bright's Disease of the Kidneys.* By T. GRAINGER STEWART, M.D., F.R.S.E., Physician to the Royal Infirmary, Lecturer on Clinical Medicine, etc. Second edition. 8vo. pp. 334. New York: William Wood & Co., 1871.

THE additions to the second edition of this excellent treatise are comprised mainly in new information on the subject of clinical history and treatment, embodied in reports of additional cases occurring in the practice of the author. The views upon the pathology announced in the first edition are continued here, the author having found no reason to depart from them in any essential particular. They are practically those of the German school of pathologists as represented by Virchow, and their repetition here may be useful.

1. The *inflammatory form*, of which there are three stages:—
   a. That of inflammation.
   b. " fatty transformation.
   c. " atrophy.
2. *The waxy or amyloid form*, of which, also, there are three stages:—
   a. That of degeneration of vessels.
   b. " secondary changes in the tubes.
   c. " atrophy.
3. *The cirrhotic, contracting, or gouty form.*

We wish our own limited experience would justify the adoption of such a sharply defined relation between the divisions of class (1) as is involved in this classification; for, if this were the case, the subject would be greatly simplified. We have, however, so constantly met cases of fatty kidney, and of contracted kidney independent of gouty history, which could in no possible manner be traced to inflammatory origin, that we have not felt justified in adopting the views of the great Berlin pathologist and his school. Dr. Stewart, however, appears to have had good opportunities for arriving at an accurate knowledge of the appearances and minute changes in the Bright's kidney, in his former position of pathologist to the Royal Infirmary of Edinburgh, and if his views are based upon observation, as they seem to be, great credit attaches to himself, and additional lustre is reflected on the name of Virchow.

We are, however, surprised that one who has had the liberal opportunities which Dr. Stewart seems to have had, should not have indicated in classification the wide difference which evidently exists not only in gross appearances, but also in the minute changes, in two forms of fatty kidney, whatever be their mode of origin. We allude to those conditions commonly implied in the terms *large white* and *yellow fatty kidney*. The appearances of these organs are quite sufficiently distinct to justify a separation even if it be made subordinate to a primary division, in which they both are included. The first of these forms, often spoken of as chronic tubal nephritis, is undoubtedly often a sequel, or, if it is preferred so to call it, a *second* stage of acute nephritis. But the second, attended by dropsy and highly albuminous urine, numerous oil-casts and much free oil, would seem often, at least, to have a different origin. This additional form, upon which we insist as sufficiently distinct, is also different from the fatty kidney, to which the author devotes a supplemental chapter, and to which he alludes on page 19 of this edition as follows: "I would only in this place remark, that while fatty degeneration undoubtedly results from inflammation and constitutes one of the most prominent features of its more advanced stages, it also occurs in the other forms of Bright's disease, and is often exceedingly pro-

nounced in cases not referable to this category at all, not being associated either with albuminuria or dropsy."

With the remainder of the classification, as embodied in (2) we have no difference, though we have never seen sufficient reason for making a separate form of the *gouty* contracted kidney as in (3), but have always made a single division of the contracting organ. We admit, however, some reason for such a separation as is made by the author. The distinction between this and the third stage of the inflammatory form lies in the quantity of interlobular connective tissue and the condition of the tubules. In the former, the contraction is mainly the result of absorption of the so-called exuded matter, much of which, however, still remains in the tubules, which are variously atrophied. In the latter, the diminution in size is due to a proliferation of the elements of the intertubular connective tissue, and subsequent contraction of the same. Dr. Stewart believes, also, that in the third stage of the inflammatory form the fibrous stroma is relatively more abundant than in health, but by no means to so great an extent as in what he terms the cirrhotic form.

With the trifling exceptions we have named, we have no fault to find with the book of Dr. Stewart. On the other hand, we know no work on the subject which is more valuable, and none which affords such pleasant reading. The style is clear, sufficiently concise, and even the reports of cases are attractive reading. We commend it most heartily. The illustrations, typography, and general appearance of the volume add to its value. J. T.

ART. XXIX.—*A Clinical Manual of the Diseases of the Ear.* By LAURENCE TURNBULL, M.D., Physician to the Department of Diseases of the Eye and Ear, of Howard Hospital, of Philadelphia; &c. With a coloured lithographic plate, and over one hundred illustrations on wood. 8vo. pp. 486. Philadelphia: J. B. Lippincott & Co., 1872.

THE plan of this work is comprehensive, and, besides the anatomy and pathology of the ear found in all the aural text-books, includes chapters on the "Physiology of Hearing," "Acoustics," and "Deaf-Mutism."

The last gives an interesting account of the different systems of teaching deaf mutes, and the principal institutions devoted to that object.

In the classification of the different forms of diseases of the ear, the author furnishes a table of six hundred cases from his own practice, in addition to a large number from other sources.

A good deal of space, perhaps more than is necessary, is devoted to the description and figuring of instruments, most of which are familiar to every practitioner. The forceps described in the list of illustrations as the "author's angular forceps," which, by the by, are not "angular," but curved, seem to be an improvement on the old form.

The book gives evidence of research; its bibliography is copious, and it presents a full summary of the subject of which it treats.

We must, however, dissent from the author's statement in the preface that "he has not simply recorded the views and opinions of others," as we have hardly been able to find throughout the volume an original observation or a new idea.

There is an unusual, probably unprecedented, profusion of quotation marks

upon its pages. In fact, it may be said to consist principally of matter that has been transferred *verbatim* from other works, and cannot, therefore, claim to be anything more than a compilation. As such, it will be found useful, particularly for convenience of reference, as the selections are numerous, and from the best authors. Indeed it might almost be called an aural encyclopædia.

The value of a medical work is by no means always in proportion to its originality, and the author has done service in condensing so much information collected from every available source.

The publishers have done their share of the work well, and the general appearance of the volume is very attractive. But the instances of awkward phraseology and careless proof-reading are unusually numerous.

Such expressions as "*Anatomical* description of the ear and its *functions*," and "*Frozen* and gouty *inflammation* of the auricle," glaring in large type in the running-title of pages, cannot fail to strike the most superficial reader; and there is scarcely a chapter that does not contain many passages that require revision. G. C. H.

ART. XXX.—*Die "Exakten" Deutschen Ohrenärzte.* Von Dr. W. KRAMER, Geheimer Sanitätsrath. 8vo. pp. 40. Berlin, 1871.
The "Exact German Aurist." By Dr. KRAMER, Sanitary Privy Councillor.

THIS publication, as the author remarks, is to be received as a kind of appendix to his well-known "Treatise on the Nature and Treatment of the Diseases of the Ear," an English translation of which was published in Philadelphia, in 1838. It presents a kind of running commentary on some of the views advanced by the more prominent of the European aurists of modern times. The criticisms of Dr. K. enforce the necessity of a careful comparison of the lesions discovered after death in each portion of the auditory apparatus, with the symptoms which accompany during life the several forms of disease of the ear, as the only sure foundation for a correct and reliable pathology and diagnosis in each, and ultimately a rational and successful plan of treatment.

The work throughout is replete with interest and practical instruction. It would be a pleasant task for us to present a full analysis of its entire contents, could we do so with justice to the author and our readers, and without encroaching upon the limits of the departments of the Journal devoted to original contributions, and to a notice of the additions constantly being made, at home and abroad, to every department of medical science. D. F. C.

ART. XXXI.—*Emergencies and how to treat them. The Etiology, Pathology, and Treatment of the Accidents, Diseases, and Cases of Poisoning, which demand prompt action. Designed for Students and Practitioners of Medicine.* By JOSEPH W. HOWE, M.D., Visiting Surgeon to Charity Hospital; Lecturer on Surgery in the Medical Department of the University of New York, etc. etc. Small 8vo. pp. 265. New York: D. Appleton & Co., 1871.

THE range of this book extends over almost all the emergencies which can occur in general practice, the object of the author being to furnish, in narrow

compass, a guide for the treatment of all the important exigencies included by our science. The measures advised by Dr. Howe are those recommended by standard authorities and such as his own experience and observation have found effectual, nevertheless, in some instances we feel obliged to differ from the views expressed.

Hemorrhage, general, special, and of the uterus, is the subject treated of in the first three chapters, and the directions given are concise and accurate, but do not require notice from us beyond drawing attention to them as such.

Chapter IV. discusses wounds of important organs, and includes those involving the throat, thorax, and abdomen, with the contained viscera. In it is also described a very rare operation, namely, ligation of the internal mammary artery, as well as the steps pursued in performing perineal section, and it contains short notices of gunshot wounds in general, and wounds of joints. In the next chapter wounds of arteries and veins are considered, and directions given for the ligation of special vessels. In speaking of the application of a ligature to the arteria innominata, the operation is said to be rarely successful, which expression hardly conveys a correct idea of the result to be anticipated from its performance, it having been invariably fatal, with one exception. Excellent advice is given—to tie all wounds of the palmar arch at the seat of injury—but no reference is made to the tediousness and difficulty of so doing, nor, indeed, is this point referred to in systematic works generally. We recollect very well the first time we had occasion to do that operation, occupying a long period in its performance, much to our own mortification, and being afterwards relieved to find that others of more mature experience had passed through similar trials. While discussing the cause of death in those cases where air has been admitted into the veins during an operation on the neck, Dr. Howe promulgates as his own the following theory: when the atmosphere, with a pressure of fifteen pounds to the square inch, is admitted into the cavities of the heart, which only possesses a propelling power of thirteen and a half pounds to the inch, the heart being overpowered, instantaneous paralysis ensues, and though the weight of the column of air may be immediately removed by closing the wound, there is no way to remove the admitted air rapidly enough for the heart to resume its action, and death ensues.

Chapter VI. contains an account of the character and treatment of dissecting wounds, hydrophobia, and the bites of snakes and insects. While enumerating the ways in which the first-named injuries may be produced, no mention is made of puncture by a needle, which, in our experience, we have found to be among the most certain methods of introducing poisonous matter into the system.

The extraction of foreign bodies forms the subject of the following chapter, laryngotomy, tracheotomy, and œsophagotomy being described in it. We regard it as a mistake to speak of these operations in similar terms and with equal brevity, for while the first two are measures evidently called for in emergency, and such as any well-educated physician should not hesitate to perform in the absence of a surgeon, œsophagotomy, on the other hand, is very rarely called for, is not generally done in haste, and involves great danger *per se*, as requiring for its performance considerable manual dexterity and very perfect anatomical knowledge. Dr. Sayre's bristle probang, for the removal of pins, bones, &c., from the gullet, is incorrectly spoken of as recently introduced, whereas it, or an instrument similar to it, has been known to us for many years. We are advised to remove insects from the ear by excluding the air with a plug of cotton moistened with vinegar; we have had no opportunity to test this plan, but should imagine it to be a much less effectual remedy than the old one of closing the spiracles of the intruder by filling the meatus with oil. The

general impression left upon our mind by a careful reading of this chapter is, that the subjects treated of in it are rather summarily dismissed.

Loss of consciousness, subdivided as coma and syncope, occupies two chapters. The causes included in the first division are enumerated to be, compression of the brain from extravasation of blood or depressed bone, embolism, uræmia, alcohol, hysteria, epilepsy, and concussion. Some general directions are given to aid the student in making a differential diagnosis, but on a subject which furnishes so ample a field for the exhibition of the niceties of diagnostic skill little can de done in the narrow limits our author allows himself. We must take exception to the advice given by Dr. Howe, to trephine in those cases where symptoms of compression are associated with a history of external violence but no evident lesion of the skull exists, as we think the adoption of such a course will often entail disappointment upon the operator, who will be quite as likely, if he finds extravasation at all, to find it associated with laceration of the brain, and effused into its substance, as to find a clot which can be wiped off the surface of the hemispheres. No mention is made under this head of that valuable adjuvant of the treatment of these cases—absolute diet. Syncope, as a cause of loss of consciousness, is considered as depending upon hemorrhage, thrombi in the pulmonary vein, anæmia, mental emotion, blows upon the epigastrium, and collapse.

Dr. Howe adheres to the term asphyxia, as most used, though less accurate than apnœa. The special causes of asphyxia considered by the author, are strangulation, compression of the chest-walls, the inhalation of poisonous gases and drowning; for each of which accidents the appropriate treatment is indicated, the inhalation of pure oxygen occupying a prominent place among the remedies proposed. The directions for the resuscitation of persons taken from the water are especially clear and simple.

Our author is more discursive than usual in Chapter XIII. and after referring to the history of sunstroke, as contained in the Old Testament, he presents the subject, as now known, with its appropriate treatment, in a succinct and forcible manner. Dyspnœa and œdema glottidis receive satisfactory notice in successive chapters; for the latter affection, when at all serious, either laryngotomy or tracheotomy is properly considered as indispensable for successful treatment. While narrating the long list of remedies which have acquired reputation in the treatment of asthmatic paroxysms, no mention is made of opium, the exhibition of which in full doses, in our own experience, has appeared worth all the rest.

The subject of convulsions occupies considerable space.

"Suspended Fœtal Animation" and "Complications of Labour" are the headings of Chapters XVII. and XVIII., the first of which appears to us to be a misnomer, for it is hardly possible to treat suspended animation in the fœtus before birth, while, after that event, it is rather hard to so style the new-born child. Chapter XIX. contains remarks upon retention of urine, dislocation of the neck, injuries from lightning, and colic. For the relief of the first affection the treatment advised inclines to be heroic, puncture of the bladder, which, in our observation, is so rarely necessary, being spoken of as an every-day resource; while the danger of attempting to reduce a dislocation of the neck is not referred to, but the operation is described in pretty much the same terms as would be appropriate in speaking of the reduction of any other luxated joint.

The five remaining chapters treat of poisons and their antidotes, but contain nothing requiring special notice from us, further than to say that the work seems well done, and the instructions given such as will meet with general endorsement.

Throughout the volume that too common fault, carelessness in writing prescriptions, is very manifest, nominative and genitive, Latin and English being mixed in sad confusion and in utter contempt of all the rules of grammar.

Several inaccuracies in the names of authors cited are also contained in the book. Many of the sentences are carelessly constructed, bearing the marks of hasty composition, and the style throughout, while forcible, is somewhat slipshod. With these exceptions the book is creditable to its author, yet we think Dr. Howe would do well either to enlarge the volume into a more complete treatise, or to cut out much of the theory it contains and reduce it to a mere handbook. For works of this latter class there always seems to be a steady demand, as they are very popular with many, but our individual feeling has always been one of distrust for all royal roads to learning, it appearing to us that prompt and efficient action in emergencies can only be looked for from those well instructed in the general principles upon which our art is based.

S. A.

ART. XXXII.—*Bromides: their Physiological Effects and Therapeutical Uses.* By ROBERTS BARTHOLOW, A.M., M.D., Prof. of Materia Medica and Therapeutics in the Medical College of Ohio. Being the Dissertation to which the Fiske Fund Prize was awarded, June 9, 1869. 8vo. pp. 47. Providence: Sidney S. Rider and Brother, 1871.

THIS essay, to which the Fiske Fund Prize was awarded, bears evidence of diligent research and careful clinical observation. In it the author studies successively the (I.) Physiological Effects of the Bromides in general; (II.) Physiological Effects of the different Bromides compared; (III.) Therapeutical Effects of the Bromides; (IV.) Applications of the Bromides in the treatment of Disease; (V.) The "Alterant" Bromides.

The part which probably will interest the general reader most is that in which the physiological effects of the different bromides are compared, for, aside from the general interest the subject has to the practising physician, it is to the elucidation of this point that Dr. Bartholow's experiments and clinical observations have been principally directed. The conclusions at which he arrives in this chapter are as follows:—

"The bromides of potassium, ammonium, and sodium generally correspond in the results and mode of their action.

"They produce weakness, trembling, and defective co-ordination of muscular movements and finally complete paralysis (destruction of irritability of muscle and sensibility of motor nerves). Bromides of potassium and ammonium are more active in causing these results than the bromide of sodium.

"They depress the action of the heart and lower the animal temperature; but the bromide of sodium is the most, and the bromide of ammonium the least, powerful in this respect.

"Each diminishes the functions of the brain, producing somnolence; but this effect is more fully induced by bromide of sodium than by the bromide of potassium, and by the latter more than by the bromide of ammonium.

"As toxic agents, bromide of potassium takes the first place, bromide of ammonium the second, and bromide of sodium the third." I. M. H.

ART. XXXIII.—*Internal Urethrotomy as a Cure for Urethral Stricture.* By C. H. MASTIN, M.D., Mobile. Written by request for the Medical Association of Alabama, Session of 1871. 8vo. pp. 34. Mobile, 1871.

AFTER an interesting historical sketch of the early applications of internal urethrotomy to the treatment of stricture, Dr. Mastin proceeds to describe the several modes of performing the operation now commonly employed, and gives representations of the various instruments devised for the purpose by the ingenuity of surgeons. The urethrotome which has proved most satisfactory in Dr. Mastin's hands is a combination of "the 'small probe-pointed silver catheter' of Sir Henry Thompson, with the triangular blade of Maisonneuve, guarded by a fixed shield." This, which he calls a "catheter urethrotome," is employed by passing the probe-pointed extremity into the bladder, while the shield rests against the anterior face of the stricture; "having satisfied ourselves that the point of the catheter is within the bladder, which will be proved by a drop of urine appearing after the withdrawal of the mandrin, and that it is not in a false passage, we have only to turn the stop screw, push on the blade, and the stricture is cut."

Dr. Mastin's success in internal urethrotomy appears to have been very great, for he has, he tells us, operated over seventy times, without once meeting with any trouble of consequence. J. A., JR.

ART. XXXIV.—*Annual Report of the Surgeon General, United States Army,* 1871.

FROM this report we glean the following interesting facts:—

"The monthly reports of sick and wounded received at this Office for the fiscal year terminating June 30, 1871, represent an annual average mean strength of 29,365 *white*, and 2608 *colored* troops.

"Among the *white* troops, the total number of cases of all kinds reported as taken on the sick list was 63,507, being at the rate of 2163 per 1000 of mean strength. (That is about two entries on sick report during the year for each man.) Of the whole number taken on sick report 54,710, or 1863 per 1000 of strength for disease alone, and 8797, or 300 per 1000 of strength were wounds, accidents, and injuries of all kinds.

"The average number constantly on sick report during the year was 1480, or 51 per 1000 of strength; of these 1190, or 41 per 1000 of strength were under treatment for disease, and 290 or 10 per 1000 of strength for wounds, accidents, and injuries.

"The total number of deaths reported was 519, or 17 per 1000 of mean strength. Of these 363, or 12 per 1000 of strength, died of disease, and 156, or 5 per 1000 of strength, of wounds, accidents, and injuries.

"The total mortality rate is greater than that for the previous year, the chief increase occurring in the proportion of deaths from disease. The proportion of deaths from all causes to cases treated was 1 death to 122 cases.

"1091 *white* soldiers are reported to have been discharged on 'Surgeon's Certificate of Disability,' being at the rate of 37 per 1000 of mean strength.

"The reports from the *colored* troops give the following figures, which do not include the white officers:—

"The total number of cases of all kinds reported was 3551, or 1362 per 1000 of strength. Of these 2964, or 1137 per 1000 of strength, were cases of disease, and 587, or 225 per 1000 of strength, were wounds, accidents, and injuries.

"The average number constantly on sick report was 104, or 40 per 1000, of whom 74, or 28 per 1000, were under treatment for disease, and 30, or 12 per 1000, for wounds, accidents and injuries.

"The number of deaths from all causes reported was 49, or 19 per 1000 of strength. Of these 28, or 11 per 1000 of strength, died of disease, and 21, or 8 per 1000 of strength, of wounds, accidents, and injuries. The proportion of deaths from all causes to cases treated was 1 to 72.

"The number of discharges on 'Surgeon's Certificate of Disability' was 71, being at the rate of 27 per 1000 of mean strength.

"There were entered on the registers the histories of 5210 surgical cases of the late war, making a total of 235,398 now recorded; also, additional information respecting 9661 cases already recorded, and prepared for revision abstracts of 8947 cases which were not placed on the permanent registers. The hospital record of 22,756 men was searched; 16,008 names were indexed. The Pension Medical Examiners' reports of the condition, at the latest dates, of mutilated men, were transcribed in 2564 instances. Histories of surgical cases were furnished to other departments of the Government in 65 instances.

"The Army Medical Museum continues to increase in the number and variety of specimens and its consequent usefulness. The number of specimens added during the year was 1516, a present total of 15,018.

"The number of visitors was over 15,000 during the year."

We are further told that the first part of the Medical and Surgical History of the War is near completion, and it is to be hoped that Congress will make an ample appropriation for the publication of the remaining parts. The publications which have been issued already from the Surgeon General's Department have proved to be most valuable contributions to the science of Surgery, and have redounded to the credit of the army medical staff, and been in the highest degree creditable to the country.

ART. XXXV.—*Complete Report of the Board of Health and Board of Consulting Physicians, as presented to the City Council, December 12th, 1871; to which is annexed Instructions for Controlling Smallpox Contagion, adopted by the Board of Health of Lowell, Mass.* 8vo. pp. 18.

FROM this very interesting report we select a few facts and admissions, inasmuch as they furnish indisputable proof of the value of vaccination, when once the human system is effectually brought under its influence, as a certain and permanent protection from the contagion of smallpox, as well as of the necessity of revaccination to insure success and as a test to determine whether an abiding influence has been secured.

"Our experience," say the Board of Consulting Physicians, "in dealing with the present epidemic, compels us to place *isolation* before vaccination. The latter has not seemed to afford that protection which has usually been ascribed to it." At particular stages of the epidemic, vaccination, the report remarks, did not arrest the progress of the disease as was expected. In individual cases it has not prevented those *apparently* well vaccinated from having a violent, and, in several instances, a fatal attack of smallpox. A careful examination of quite a number of very grave cases at the hospitals distinctly showed marked cicatrices—some large and well pitted—the result, as the patients said, of *inoculation for smallpox* itself, or of vaccination with the virus of cow-pox. Still there were, it is admitted, other patients in whom the disease was much modified—rendered lighter in form or shorter in duration—by the

effects of vaccination. There were many instances, also, of individuals—including several infants—subjected to the greatest exposure, but who had recently been successfully vaccinated, remaining perfectly protected. The most striking proof of the protective power of vaccination was found among the employés of the mill corporations. Under the direction of the agents of these corporations, a more systematic and thorough course of vaccination has been pursued among the operatives and others connected with the mills, than among the other inhabitants of the city of Lowell. The rule has also always been to remove at once every case of smallpox or varioloid to the corporation hospital as soon as discovered. From the commencement of the epidemic in February, 1871, we are told that only *forty-nine* patients had been so sent from the mills and their boarding-houses, and in no instance have other parties exposed at the time, taken the disease. Twenty of the forty-nine were females, and only two died. It is true, however, that some of the operatives not residing in the corporation boarding-houses had been taken to the City Hospital, and others, prior to September 25th, were treated at their own houses, but these were few in number. As the persons connected in some way with the mills comprise one-third or more of the population of the city, the proportionate number sick with smallpox or varioloid from this class has been, hence, relatively, very small.

The good results derived from the uniform and prompt removal from the mill corporations of all cases of smallpox and varioloid, of course speak in favour of isolation as a prophylactic. Another striking fact, however, in favour of the protective power of vaccination under exposure, is deducible from the fact that very few school-children contracted the smallpox; the rule for admission into the public schools requiring evidence of successful vaccination having been always rigidly enforced. While the Board of Physicians were constrained to place *isolation* as the more successful agency in arresting the spread of the epidemic, they would by no means be supposed to undervalue the importance of vaccination, "*when properly performed.*" Of two things the members of the Board declare themselves more fully convinced than ever, namely: 1st. Of the frequent imperfection of vaccination, either in the manner of its performance, or of some defect in the virus employed; and 2d. Of the "*absolute necessity of revaccination.*" The latter is indispensable, whether we admit that the protective efficacy of the vaccine prophylaxis gradually diminishes with the normal changes that take place in the human body, as it advances in age (and therefore revaccination at proper intervals becomes necessary to test its continuance); or that, often, the first or even subsequent vaccinations, may not have produced a complete prophylaxis, and therefore, the operation should be repeated again and again, until it ceases to produce any effect, "when," the Board admits, "its protective power will *continue for life.*" D. F. C.

ART. XXXVI.—*Lessons on Population, suggested by Grecian and Roman History.* By NATHAN ALLEN, M.D., Lowell, Mass. 8vo. pp. 16. Boston, 1871.

IN this well-written and highly-suggestive pamphlet, Dr. Allen has examined the lessons on population, based upon facts deducible from the history of the downfall of ancient Greece and Rome. The substance of the essay appeared originally in the *Congregational Quarterly* for October, 1871. Dr. Allen

shows, with great clearness, positive evidence in support of his views, advanced in preceding publications, in respect to population, namely, that when this is dependent mainly upon natural human increase, it must necessarily become diminished by the prevalence of causes having a tendency to render prominent in a people a morbid excess of the nervous temperament. When the great mass of any community is seeking after an extravagantly high standard of self-comfort as their "supreme good," with all the energy, perseverance, and ingenuity they can command, everything standing in the way of its attainment must and will be sacrificed, even including marriage and parental relationship, if these be thought in any degree to conflict with it.

The lessons, based upon the teachings of Grecian and Roman history, Dr. A. would apply to the older settlements of our own country upon its Atlantic border. "As a people," he remarks, "we have set up a high standard of comfort—extravagant, and too expensive; it has too many wants, and requires such an amount of physical stamina and brain-power, as to result in premature exhaustion. This standard is based too much on mere wealth and the selfish nature of man; it is not calculated to develop harmoniously, or in the most healthy manner, all parts of the human body; nor to aid as it should in developing the moral and religious character of man, in accordance with the revealed will of God. The results of such a type of organization were determined, in the case of Greece and Rome, near two thousand years ago, the former flourishing some six hundred years, and the latter five hundred. It is now two hundred and fifty years since the first settlement of New England, and as a people, we are already reaching a crisis, a culminating point in history, where it is becoming a question whether there is, from year to year, an actual increase or not of native population. And if a decline once commence, the *decrease* may be rapid. We cannot well deceive ourselves if we would, for there are agents or causes working gradually and quietly, which seriously threaten the best interests of our people." D. F. C.

ART. XXXVII.—*A Practical Treatise on the Diseases of Infancy and Childhood.* By THOMAS HAWKES TANNER, M.D., F.L.S., etc. *Third American Edition, from the Last London Edition, Revised and Enlarged,* by ALFRED MEADOWS, M.D., London, Member of the Royal College of Physicians, etc. etc. 8vo. pp. 560. Philadelphia: Lindsay & Blakiston, 1871.

DR. TANNER being, from the press of other engagements, rendered unable to superintend the publication of the second edition of his treatise, entrusted the task to his friend, Dr. Meadows, by whom it has been most successfully accomplished. By working out the author's design and intention, he has rendered the work, in the edition before us, an easy and reliable book of reference for the busy practitioner. Dr. Meadows has seen fit to entirely change the general plan of the treatise. He has divided it into four parts. The *first* treats of the physiology and pathology of childhood, including the anatomical peculiarities of childhood, with its hygiene, the process of dentition, etc. The *second* part treats of general diseases, including all the varieties of fever, continued and eruptive, and the several diathetic affections—scrofula, tuberculosis, rickets, and syphilis. The *third* part includes a consideration of the special diseases of children, arranged in physiological order, viz., those of the nervous,

respiratory, circulatory, digestive, and urinary systems respectively; also, diseases of the skin, eye, and ear. In the *fourth* part are discussed the more common accidents, injuries, malformations, and deformities—congenital and acquired—of childhood, including those connected with birth. The *appendix* of formulæ has been enlarged and rearranged. The editor calls especial attention to the views advocated by him in regard to diathesis and its importance in the therapeutics of infancy and childhood. Long experience and careful observation have taught him that herein will be found the key to the successful treatment of the diseases of early life. D. F. C.

ART. XXXVIII.—*The Management of Infancy, Physiological and Moral. Intended Chiefly for the Use of Parents.* By ANDREW COMBE, M.D. *Revised and Edited by* SIR JAMES CLARK, BART., K.C.B., M.D., F.R.S., Physician-in-ordinary to the Queen. First American from the tenth London Edition. 12mo. pp. 302. New York: Appleton & Co., 1871.

OF the character and nature of the work of Dr. Combe, nothing need be said, since it is too widely known and too highly appreciated to require any comment at this late date. By the editor, the order of some of the chapters has been altered, with the view of bringing the subjects treated of more consecutively before the reader. Some portions, especially in the earlier chapters, have been omitted, as less necessary now than during the author's lifetime. Some additional information has been given, on the causes and extent of infant mortality, chiefly taken from recent reports of the Registrar-General, while an appendix has been added, containing some useful matter not exactly fitted for the body of the work. D. F. C.

ART. XXXIX.—*Essentials of the Principles and Practice of Medicine. A Handbook for Students and Practitioners.* By HENRY HARTSHORNE, A.M., M.D., Professor of Hygiene in the University of Pennsylvania, etc. etc. Third Edition, Thoroughly Revised. 12mo. pp. 487. Philadelphia: H. C. Lea, 1871.

WE are pleased to greet a new edition of this very able handbook of pathology and therapeutics. It is one of the best of its class with which we are acquainted, forming a useful and safe guide to the advanced student, and a valuable "book of remembrance" to the young practitioner.

The present edition bears evidence to the "great pains that have been taken by the author in its preparation, to supply omissions, and to add whatever has seemed to be most positive and important in the recent advances of medical science." Such additions occur at the greatest length in connection with the topics of tuberculosis, relapsing fever, and therapeutic uses of carbolic acid and hydrate of chloral. D. F. C.

QUARTERLY SUMMARY

OF THE

IMPROVEMENTS AND DISCOVERIES

IN THE

MEDICAL SCIENCES.

ANATOMY AND PHYSIOLOGY.

1. *On the Physical Principles by which the Escape of the White Corpuscles of the Blood through the Capillaries is effected.*—The remarkable corroboration that the statements of Waller and Cohnheim respecting the passage of the white corpuscles of the blood through the walls of the capillaries have received by physiologists generally, and the important bearing these observations have upon the question of the formation of pus and the fibrinous exudation of inflammation, confer special interest on all experiments and investigations tending to elucidate the physical principles by which the escape of these corpuscles is effected. At first sight nothing could well be conceived more improbable than that the formed or morphological elements of a fluid contained in a system of elastic tubes should be able to make their way out, whilst the limpid fluid itself should be retained. Not only is it difficult to imagine by what agency the first tendency to pierce the wall is brought about in the corpuscles in the absence of any visible holes in the membrane, but it is still more difficult to explain why the rest of the fluid does not pursue the same path. It is clear that it cannot be exclusively attributed to variations in the pressure of the fluid against the walls of the vessels caused by the action of the heart; for, as this is exerted upon the fluid as a whole, it is inconceivable that it should cause the extrusion of certain particles alone, without the operation of some other force.

An inquiry into the nature of this force has lately been undertaken by Dr. Richard Norris, of Birmingham, who has published two valuable papers upon it: one of which appears in the *Proceedings of the Royal Society*, the other in the *Transactions of the St. Andrews Medical Graduates' Association*. Dr. Norris points out that all the membranes which enter into the animal body may, from a physical point of view, be divided into two orders: the very fine, structureless, homogeneous films, which must be regarded as simple *cohesion membranes*, in contradistinction to the second order of coarse membranes, to which certain mechanical arrangements—as interlacing fibres—are superadded, which have the effect of increasing the strength. Films of collodion, gelatine, India-rubber, and soap may be regarded as examples of the first class of membranes; and these are susceptible of two states—the fixed or rigid condition, and the contractile or elastic state, dependent upon the presence of the principle of "flow," which principle may be operative in every shade and degree, from perfect liquidity to absolute rigidity. Dr. Norris takes the soap film as the best illustration of the class of homogeneous cohesion films possessing in the greatest perfection this principle of "flow," and as best exhibiting pheno-

mena which he has generalized under the term "progressive cohesive attraction." He describes the production of a soap-bubble, and points out the capability it possesses, under the influence of progressive adhesion, of perfecting any absence of continuity that may exist in its structure; so that after its complete detachment it may be perforated by any wetted body, which may again be withdrawn without the bursting of the sphere. Again, he shows that if a soap-bubble be allowed to impinge on a smooth rigid surface, it becomes so drawn down to the plate as to assume a hemispherical form; but if for the rigid surface a delicate plastic film be substituted—such as may be taken up by a ring from a solution of soap—the soap-sphere immediately *penetrates* the film, and arranges itself so that half is on one side and half on the other. If pressure be now applied to one side, the bubble can be caused to protrude still further through the film, and it may even ultimately be made to pass through the film without any rupture occurring in either.

Without entering further into Dr. Norris's ingenious experiments, it is obvious that we have here a very close similarity to the phenomena accompanying the extrusion of the white corpuscles. In both cases the conditions essential for a rigid or a plastic body to pass through a colloid film are present; and these he has shown to be—firstly, an intimate power of cohesion, either mediately or immediately, between the film and the body; secondly, a certain amount of pressure from within; thirdly, power in the substance of the film to cohere to the surface of the body during its passage; and, lastly, cohesive plasticity of the particles of the material of which the film is itself composed, so that the breach in it may again become united as it descends upon the opposite surface of the body which is being extruded. It may be asked, however, why the white corpuscles are not constantly escaping in large numbers from the vessels; to which Dr. Norris replies, that as it is only under certain conditions that the soap-spheres attract each other—namely, when free liquid is cohering to their surfaces—so with the corpuscles, which do not unite with each other or with the capillary wall unless their normal osmotic relations are disturbed, the exosmotic current setting in excessively causing their surface to become coated with previously-contained substance, when they become instantly attractive of the capillary wall.—*Lancet*, Jan. 6, 1872.

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2. *Case of Doubtful Sex.*—Dr. FLUME describes, in the *Nassauisches Aerztl. Corresp. Blt.*, No. 8, 1871, the case of an individual that, from the decided masculine type of the head and of the facial expression, the deep tone of the voice, the full, strong beard, the small, hairy breast, with entire absence of protuberant mammæ, and especially from the appearance of the external genitalia, would be at once taken for a male. A growth of hair extended from the pit of the stomach to the pubes along the median line; from the pubes, a deep sulcus descended, bounded on each side by a firm projection, covered with hair, resembling the labia pudendi of the female. From beneath the anterior commissure formed by these lateral protuberances there projected a penis, one inch in length, resembling in shape and bulk that of a full-grown man; its glans did not present any opening at its lower angle, the orifice of the urethra being at the root of the penis. The individual had never menstruated. Death occurred from an attack of pleurisy. An examination of the body showed the presence of a uterus of the usual size and with a cavity the same as of the normal organ. The tubes and ovaries were also present. The pelvis, in its conformation and expansion was that peculiar to the female. The presence or absence of this formation of pelvis is pointed to by Dr. F. as of the greatest importance in determining the actual sex in all doubtful cases, while the case shows the unreliability of the test referred to by Casper, the presence in the male of the strip of hair along the linea alba.—*Centralblatt f. d. Med. Wissenschaften*, Dec. 9, 1871. D. F. C.

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3. *On the Nutrition of Muscular and Pulmonary Tissues in Health and in Consumption, with Remarks on the Colloid Condition of Matter.*—This is the title of a very interesting paper by Dr. WILLIAM MARCET, published in the

Edinburgh Medical Journal for February last. The following are the conclusions given by the author as the result of his elaborate researches:—

1. Phosphoric acid and potash may be prepared artificially in the colloid state, by dialyzing a mixture of chloride of potassium and phosphate of soda.

2. Wheaten flour, potato, and rice are found to contain, respectively, nearly the same proportions of colloid phosphoric acid and colloid potash, compared to the total quantities of these substances present; and these same proportions of phosphoric acid and potash are occasionally found to exist also in blood.

3. Plants form colloid material, although they may find some ready prepared, or in process of preparation in the soil.

4. Muscular tissue in health is formed of three classes of substances: (1.) Those which constitute the tissue proper; (2.) Those destined to become transformed into the tissue proper, and make up for the waste; (3.) Those which are in process of elimination—the first being solid and colloid, the second fluid and colloid, and the third soluble and crystalloid; the phosphoric acid and potash in the third class of substances occur precisely in the proportions required to form crystalloid pyrophosphate of potash. This is invariably true for the flesh of animals highest in the scale; but in the salmon the proportions do not quite agree with those of the above compound, which appears to show that the material in progress of elimination is somewhat less crystalloid in fishes than in the flesh of the higher animals, and this would account for an accumulation of effete matter in the salmon.

5. Blood-corpuscles appear to take up albumen, phosphoric acid, and potash in the blood, and yield them in the proper proportions to muscular tissue for its nutrition; but this subject requires further investigation.

6. The nutrition of pulmonary tissue in health differs from that of muscular tissue, inasmuch as the proportion of phosphoric acid to the albumen in the tissue proper, and consequently also in the nutritive material, is much higher in the lungs than in flesh; and that of the potash in the effete material is much higher, proportionally to the phosphoric acid, in pulmonary than in muscular tissue. This excess of potash is apparently eliminated under the form of carbonate.

7. The nature of the chemical changes which take place within muscles in consumption is the same as in health, but these changes are lessened in degree, the amount of nutritive material supplied being diminished. Moreover there appears to be in muscular tissue in phthisis a beginning of that separation of water from the solids which, under other circumstances, only occurs some time after death.

8. Muscular tissue in consumption contains more soda and chlorine than in health; in the mean proportion of 0.117 of chlorine and 0.239 of soda in health, to 0.385 of chlorine and 0.446 of soda in consumption, for 200 grammes of flesh; showing apparently that the physical power or diffusion which had been kept in abeyance in health begins to act in phthisis.

9. The pulmonary tissue in phthisis when consolidated and softening, still undergoes a process of nutrition; but this phenomenon is different from that which occurs in health, and becomes remarkably like the nutrition of muscular tissue.

10. The pulpy state of tubercular lungs in the softening condition appears due to an altered relation between the water and solids, and not to a fatty degeneration. The diseased organs, moreover, contain less colloid and more effete or crystalloid material than in health, these several phenomena showing, as in the case of muscles, a commencement of physical change.

11. Finally, death from consumption, when not due to asphyxia from deficient action of the organs of respiration, is apparently owing to the physical power of matter overcoming the phenomena of life, the nature of which is still a mystery, physical changes actually commencing before life is extinct.

MATERIA MEDICA AND GENERAL THERAPEUTICS AND PHARMACY.

4. *The Physiological Action of Digitalis on the Depressor Centres of Reflex Action of the Frog, along with Experiments on the Influence of the Circulation on this Organ.* By A. WEIL, M.D.—Certain phenomena have for some time been taken to indicate that in the brain there exist centres which exercise a depressing influence on reflex action. These phenomena are the increase of reflex action in beheaded animals, and also the power which we possess of voluntarily preventing reflex action. With a view to find the seat of these depressor centres, Setschenow conducted certain experiments, the result of which was to show that they are situated in the optic lobes and corpora quadrigemina. The present paper has reference to the action of digitalis on these centres, and also the influence which various changes in the circulation produce on them. The experiments are very elaborate, and are marked by great ingenuity, and the general results deduced may be stated as follows: digitalis reduces the power of reflex action in frogs which had been specially prepared to exhibit reflex action—the degree of this action being tested by the number of seconds during which the leg of such a frog was retained in an acid solution. A similar reduction of the power of reflex action is produced by depriving the animal of blood, also by stopping the heart's action, and to a less degree by a retardation of the heart's action. It was also produced by cutting out the lungs, by placing the animal for some time in an atmosphere of hydrogen, or by poisoning with HS. In these two classes of experiments the depression of reflex action is probably due to the absence of O in the blood, the depression centres being irritated by blood deprived of oxygen. The question then comes, whether the depressing action of digitalis is due to the action of this substance on the circulation, the diminution of the heart's action being the actual cause of the reduction of the reflex action. The experiments conducted with this view seem to show that while the depression is partly due to this latter cause, yet that digitalis has also a direct action on the depressor centres, this being shown by the fact that the depression is much greater than is produced by an equivalent simple diminution of the heart's action, and also that the depression sometimes precedes the retardation of the heart's action. While digitalis acts on the depressor centres, it appears also, after a certain time, to act directly on the reflex centres on the spinal cord. When small doses had been given, and a short time had elapsed, reflex action was recovered, on the removal of the depressor centres by decapitation of the frog, but with larger doses, and after a longer time, recovery did not take place, so that the digitalis had acted on the spinal cord.—*Glasgow Med. Journ.*, Feb. 1872, from *Reichert and Du Bois-Reymond's Archiv*, 1871.

5. *Physiological and Therapeutical Actions of Quinia.*—Our knowledge of the physiological and therapeutical actions of this invaluable alkaloid has been recently much extended by the labours of Binz, Ranke, Kerner, Zuntz, Scharrenbroich, and Schulte. We propose to lay before our readers a *résumé* of the chief results obtained.

Binz finds that quinia has the power of arresting the processes of putrefaction and fermentation in a high degree, and that it is an active poison for all low organisms, animal and vegetable. According to Cohnheim's views, pus, being mainly a collection of white blood-globules, which have passed through the walls of the vessels—further, quinia having the power of arresting the motions of the white corpuscles, and hence preventing their exit from the vessels—the alkaloid arrests, or at all events diminishes, the formation of pus during the course of inflammation. Moreover, it destroys the ozonizing power of certain substances; and as the red corpuscles have this power, quinia in the blood probably diminishes oxidation of tissue, and lessens the production of heat. Ranke and Kerner, indeed, have found that quinia in large doses diminishes tissue changes, as is shown by the smaller quantities of urea and uric

acid excreted; and there are many observations to show that in fevers it produces a decrement in temperature. Ranke and Kerner's experiments do not show, however, how far the lessening of tissue-waste is due to the direct action of quinia on oxidation, and how far to the indirect action of the alkaloid through the nervous system. Two methods have been employed for ascertaining the direct influence of quinia on oxidation. Harley added quinia to the blood, and found that this, when so treated, took up less oxygen and gave off less carbonic acid than blood which had not been so treated. This method is inconvenient of application, and liable to error. Zuntz employed the changes in the alkalinity of the blood for arriving at the same results. Schulte has extended these researches. If fresh blood be drawn, a development of acid begins in it, and continues, at first rapidly, then more slowly, till putrefaction sets in. Of course this acidification depends on oxidations; and the diminished alkalinity of the blood, thereby produced, furnishes a test of the rapidity with which oxidation proceeds. Schulte has confirmed the observation first made by Zuntz and Scharrenbroich, that quinia and berberine lessen the production of acid. Harley's observation is thus confirmed. Cinchonia produces similar results to quinia, though in a very inferior degree. Picrate of sodium is nearly as powerful as quinia. Zuntz found, as Ranke and Kerner had previously done, that quinia in ten-grain doses lessens the daily excretion of urea by one-third or more. Unruh has found the same to occur when quinia is administered in fevers; but his observations are open to objection. Binz's experiments are curious, and show, that, when putrefying liquids are injected into the circulation, the temperature of the body rises; but, if the fluids be previously mixed with quinia, whereby the putrefactive processes are arrested or destroyed, the rise in temperature is either entirely arrested or considerably diminished.

We think that these experiments have an important bearing on practice, and that they are in accordance with the teachings of clinical observation. It has been too much the fashion to assume that the therapeutical actions of quinia are entirely different from its effects when administered in health. We apprehend that the true method of commencing the study of the actions of medicines is first to ascertain their effects in health; then to observe their results in disease.

Dr. Crace Calvert has also recently announced the discovery of the power of quinia in preventing the development of fungi. He appears, however, to have been unaware of Binz's previous publication of the fact.—*Med. Times and Gazette*, Feb. 3, 1872.

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6. *Therapeutic Uses of Chloral.*—Dr. Oscar Liebreich, in the third edition of his treatise on hydrate of chloral, gives a general view of the classes of disease in which observation has allowed a general opinion to be formed as to the merits of the remedy. Numerous experiments in all countries have established the fact that chloral has the property of producing sleep in all pathological states where it is desirable to obtain this; and it does this without giving rise to any mischievous results. Some special peculiarities with regard to its action have, however, been observed. In a case of gout, for instance, a dose of hydrate of chloral produced excitement; but, when the patient had been treated with carbonate of soda for a week, the same dose acted as a hypnotic. This, according to Dr. Liebreich, was due to the circumstance that, at first, the formation of urate of soda deprived the blood of its normal amount of alkali, and thus prevented the transformation of the chloral into chloroform. On the other hand, and in accordance also with the theory of the transformation of chloral, it has been noticed that in typhus, where there is an excess of alkali in the blood, small doses of chloral readily produced sleep, while larger (even moderate) quantities gave rise to symptoms of poisoning. With regard to the use of chloral in operative surgery, the results of Dr. Liebreich's experiments have led him to expect with certainty that the drug may be used in such a way as to produce sufficient anæsthesia for even severe operations on the human subject. His observations of animals have shown him that there is a marked difference between a poisonous dose and the quantity sufficient to produce complete anæsthesia; and this meets the objection to chloral as compared with chloro-

form, that it is not introduced gradually into the system, but at once. Although experiments have shown that small doses of chloral have little influence on the circulation, Dr. Liebreich advises caution as to its use in heart-disease. In trismus and tetanus larger doses are indicated, as small quantities do not produce the necessary action on the spinal cord. Hydrate of chloral has been found to act beneficially in a number of cases of puerperal convulsions; and Dr. Liebreich is disposed to explain this by accepting Frerichs' theory, that the convulsive attacks are connected with the transformations of urea into urate of ammonia, and by supposing that, besides the production of chloroform, there is a formation of hydrochloric acid which neutralizes the ammonia. Among other diseases in which there has been a general agreement of opinion as to the beneficial effects of hydrate of chloral, Dr. Liebreich mentions senile insomnia, delirium tremens, nervous asthma, chorea, dental convulsions in children, seasickness, etc.—*Brit. Med. Journal*, March 2, 1872.

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7. *The Calomel Vapour Bath.*—Mr. HENRY LEE made a communication to the Medical Society of London (Jan. 29, 1872) on some cautions to be observed in the use of calomel vapour baths. He had introduced calomel baths to the notice of the profession; and before doing so he had laboured to find out the cause of failure of the cinnabar and gray oxide of mercury. Cinnabar was decomposed by heat, and gave off sulphurous acid, which irritated the lungs. The gray oxide was decomposed, and, absorbing oxygen, became the binoxide, and acted more powerfully. These remedies acted sometimes with small, sometimes with great, effect. Calomel, when used with water as he recommended, was, he thought, perfect; the skin was acted upon and the lungs were not irritated. The caution given to use water with the bath had been neglected in some quarters. During the fumigation with calomel, hydrochloric acid was given off, which was very irritating to the lungs; but no bad results followed if water were used. A young woman in good health and well nourished was suffering from a syphilitic ulcer of the throat. Twenty grains of calomel were volatilized in a teapot (without water) and inhaled. The skin suddenly became cold and livid, the lips blue, and the pulse small. The woman died, and at the necropsy the internal organs were found healthy, but the lungs were emphysematous, and there was effusion into them. A man inhaled thirty grains of calomel from a teapot for four nights for the cure of a hoarseness which had lasted two years, and had resisted a great variety of treatment. The hoarseness was cured without salivation, but the man had suffered ever since from a cough and pain in the chest. The lungs were not diseased, and no tubercle was present.—The President asked Mr. Lee if he had met with any untoward results after his treatment with the bath? What recommendation did he give to those about to use it?—Mr. Lee had not seen bad results except from pre-existing disease—old cases of mercurialization or disease of the bones. He did not salivate, but produced a slight tenderness of the gums. He used an apparatus of tin, with a tray for the calomel and a gutter for the water, a spirit-lamp to burn an ounce of spirit, half a drachm of calomel, a stool or chair, and a cloak. The calomel was sublimed and deposited on the skin. A little of the vapour was inhaled from time to time. The patient was ordered to sleep in the cloak. In four or five nights the gums were affected. In the case of one patient, two successive baths produced tender gums. Substances likely to produce diarrhœa were to be avoided.—*British Medical Journal*, February 24, 1872.

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8. *Crystallized Digitaline.*—M. BUIGNET, in a report to the French Academy of Medicine, made January 23, on the essays sent for competition for the Orfila Prize, announces that the successful candidate (whose name is withheld until the time for the official declaration arrives) has made a discovery likely to be of very great utility in therapeutics and physiology. It is the production of crystallized "digitaline" in a state of absolute purity. The product has been submitted to a rigorous examination by the Prize Committee, and the superiority of the procedure adopted for the isolation of this active principle is admitted without hesitation. Splendid crystals resembling those of sulphate of quinia, and furnishing a bright emerald green when treated by hydrochloric

acid, were exhibited at the last meeting of the Academy, and greatly admired. The chemical perfection of the product has been confirmed by its physiological and therapeutical effects in the hands of MM. Vulpian and Marrotte. Its promptitude and intensity of action are far greater than is the case with the digitaline of Homolle and Quévenne—three milligrammes administered in twenty-four hours producing saturation and intolerance, and one milligramme daily being ill-supported by most patients, so that more than half this quantity cannot usually be given. It is evidently an agent of tremendous power, which will require great caution in its employment, and may prove a fearful weapon in the hands of the poisoner.—*Med. Times and Gaz.*, Feb. 3, 1872.

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9. *Bichloride of Methylene.*—Mr. F. Searle adds his testimony in favour of this anæsthetic. In 1870 he administered it to more than 100 cases in the West of England Eye Infirmary, and has since used it in private practice. He has given it in all ages, from 6 months to 70 years, without a single cause for alarm, vomiting even being exceptional. It is important not to allow the patient to escape from its first influence, otherwise excitement ensues. (*Lancet*, May 27.) In Padua, bichloride of methylene has been employed for three years in the surgical clinic to the exclusion of ether and chloroform, being sent over from London by Messrs. Robin. Out of 108 operations performed under its influence, Dr. Rossi states that 52 patients slept tranquilly without any muscular agitation within from 1 to 5 minutes; 32 experienced slight excitement and became insensible in 8 or 10 minutes. Four only were violently agitated, and sleep was not induced from 15 to 20 minutes; 20 remained completely free from its influence even after 50 minutes of inhalation. Vomiting occurred in 8 instances. No other accident declared itself, and the liquid excited no cough, but slight lachrymation. The pulse and respiration were increased in frequency from the first, but soon returned to the normal state, and even fell below it. The face underwent no change of colour. (*Journ. de Pharm. et de Chim.*, Sept.) These results correspond in many particulars with those previously attained in England by Mr. Miall and Mr. Gaine, and this anæsthetic seems well worthy of future trial.

To the three *deaths* already recorded as happening under its use must now be added another from the Radcliffe Infirmary, Oxford. Bichloride of methylene was administered on a flannel bag to a woman, aged forty-four, who was about to undergo an operation for cancer of the breast. After two or three convulsive gasps the patient died, though the quantity administered was small. —*Dub. Journ. Med. Sci.*, Feb. 1872.

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10. *Glycerine as a Solvent for Substances used in Subcutaneous Injection.* —At a late meeting of the K. K. Gesellschaft der Aerzte in Vienna, Dr. Moritz Rosenthal, one of the *docenten* in the Allgemeine Krankenhaus, advocated the use of glycerine as a medium for the solution of the various substances used for subcutaneous injection, and also exhibited specimens of such solutions. It is of the highest importance that the glycerine used be chemically pure, and free from all traces of fatty acids. By gradual elevation of temperature, the glycerine can be made to take up into solution a large number of certain alkaloids—*e. g.*, chinin, morphin, curan, and also opium extract, and to retain them dissolved for, at all events, a year (this being the duration of the experiment) in a complete and clear solution. Certain other substances—*e. g.*, thein, caffein, and preparations of iron, are, on the cooling of the solution, reprecipitated.

Of disulphate of chinin, twenty grains are soluble in one drachm of glycerine, this being in the proportion of 1 to 3. The same amount of the solvent will take up ten grains of muriate of morphia or of opium extract. Corrosive sublimate, of which five to six grains are soluble in one drachm of glycerine, must be diluted with water. One drachm of the solvent will take up only one grain of curare. The glycerine solutions of chinin, subcutaneously applied, were found of especial benefit in intermittents.

All of these solutions were stated to remain during the summer months perfectly pure and free from all traces of vegetable moulds.

In cases where the patient is very sensitive, these solutions can be diluted

with water, and are stated to be especially adapted for injections in the region of the back, of the sides of the chest, and of the nates.—*The Lancet*, Jan. 27, 1872.

11. *Effects of Subcutaneous Injection of Morphia.*—HAUSMANN (*Berichte des Naturwis.-Medic. Vereines*) has observed—as Nussbaum and Mühe had previously done—that the subcutaneous injection of morphia is sometimes suddenly followed by peculiar effects. These consist in pain and redness of the face, contractions of the muscles of the lower jaw, a hammering, frequent pulse (130), dyspnœa, and clonic spasms of the limbs. These symptoms last five minutes. The spasms first cease; then the pulse becomes quiet, and at last violent sweating breaks out. Hausmann is disposed to accept the explanation given by Nussbaum, that in such cases the morphia directly enters the veins. The production of the phenomena does not appear to depend on the quantity of morphia injected.—*Brit. Med. Journal*, March 2, 1872.

12. *Eucalyptus Globulus as an Antiperiodic.*—At a recent meeting of the Königsberg Society for Scientific Medicine, Herr Bohn said that his observations had proved the antipyretic action of eucalyptus globulus in hectic and intermittent fever. In one case of the former it was more efficacious than quinia. In a case of masked intermittent, assuming the form of cerebral disorder, in a child seven years old, he had arrested the disease in four hours by giving fifteen grammes of the tincture in drachm doses. He expressed the opinion that the tincture would be found useful in puerperal fever of an intermittent type.—*Brit. Med. Journal*, March 2, 1872, from *Berliner Klin. Wochenschr.*, 26th Feb. 1872.

13. *Chronic Chloroform Narcosis.*—In an interesting paper (*Practitioner*, Dec. 1871), Dr. ANSTIE states that the constant use of large doses of chloroform, for the purpose of relieving pain, may bring about a degraded condition of the nervous centres analogous to that induced by alcoholic excess. Alcohol and chloroform are strictly analogous with each other, though different in all the phases of their action. Sulphuric ether is similarly analogous to both. They are all three pure *stimulants* (*i. e.* restorers of calm natural function) in small doses, *inebriants* when given in larger quantities, and *anæsthetic narcotics* in full doses. The most important peculiarity of chloroform, as compared with alcohol especially, is the fact that its great insolubility in the serum of the blood causes it to escape entirely, or almost entirely, from the lungs within a very short time (one hour?) after its administration, and without undergoing decomposition in the body. As a result of this latter fact, chloroform, however dangerous at the moment of surgical narcotization, is exceedingly free from tendencies to produce after-mischief, since the organism is speedily rid of its presence.

Unfortunately, the knowledge of this latter familiar fact has led to unwarrantable inferences as to the effects of a kind of chloroform administration which is radically different from the ordinary induction of surgical anæsthesia.

Dr. Anstie denounces the practice of patients keeping a chloroform bottle at hand and using two, three, or four ounces of chloroform many times a day, according to the promptings of pain or other nervous distress. Independent of the sudden and fatal chloroform narcosis which has several times resulted, this practice can lead to chronic mischief similar to that produced by alcoholic excess. In some cases Dr. A. has observed a state of peculiar maniacal excitement, which really deserved the name of maniacal. Another consequence not unfrequently observed, of the profuse and continued employment of chloroform, is the disposition to recurrent vomiting. The patient sometimes begins by losing all appetite; but this is not always the case. Most commonly the tendency to sickness shows itself in a disposition to retch, or actually vomit, after awakening from a heavy semi-comatose sleep: but the vomiting sometimes becomes frequent as the case proceeds, and greatly prostrates the strength. Entire intermission of the use of chloroform then becomes an absolute necessity, and the sickness usually subsides quickly.

Another phenomenon, which strikingly recalls the condition of chronic alcoholic patients, is an intense insomnia which is occasionally observed as a result of long-continued and copious use of chloroform.

The worst result that Dr. A. has seen from prolonged abuse of chloroform has been in cases of "spinal irritation" of the truly hysteric type. In this class of patients, especially in women near the menopause, he has known it to produce violent erotic excitement, to which no doubt there was a tendency resulting from the physiological state, but which was indefinitely aggravated by the injudicious use of chloroform; the proof being that it diminished, or even practically ceased, immediately that the drug was discontinued. Further, the habitual use of chloroform produces in a certain number of instances, degenerative changes which accurately imitate the degradation of tissues produced by chronic drink: at least, such is my distinct belief. Although in circumstances of ordinary chloroform narcosis, induced once for a definite surgical purpose, there seems no doubt that the whole of the anæsthetic completely leaves the organism by exhalation within an hour or two, it is probable that this is not the case where the patient is constantly saturated with the drug in frequent repetitions every day of his life. What the further chemistry of the process is we cannot at present say: it is impossible to tell whether the chloroform does or does not become oxidized within the body, as alcohol certainly does. But of the positive character of the general vital result I think there can be little doubt: quite apart from the damage to appetite and digestion which accrues, there is apparently a visible increase in the rapidity of those degenerative changes which give the organism the characteristics of old age.

Dr. A. cautions the profession against recommending chloric ether (which is simply alcoholic spirit impregnated with chloroform) as a remedy against depression of spirits, nervousness, &c. "It is just the kind of remedy," he says, "likely to be taken lavishly and thoughtlessly for such distress, more particularly, as women suffer at the menstrual period: and there ought to be no disguising the fact, that its use under such circumstances, without strict medical regulation and control, is likely to degenerate into a kind of tippling only a little less mischievous and degrading than ordinary alcoholic excess.

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14. *Elimination of Alcohol.*—Dr. A. Dupré, of Westminster Hospital, has recently presented a very important paper to the Royal Society "On the Elimination of Alcohol." Obviously, one of three results may follow the ingestion of that liquid—either all the alcohol may be oxidized, and none eliminated unchanged; or a portion only may be oxidized, and the rest eliminated unaltered; or all the alcohol may be eliminated unchanged. If all the alcohol be eliminated unaltered, it follows that if a certain amount of alcohol be administered daily, the quantity eliminated would increase from day to day, till eventually a state of equilibrium would be attained, and the amount eliminated each day would equal the amount ingested. If, on the contrary, all the alcohol were either oxidized or eliminated within a period of twenty-four hours, no increase in the daily elimination will take place as a consequence of the alcohol diet. The author undertook two series of experiments, in which the quantity of alcohol eliminated by both kidneys and lungs was determined. His results are thus summed up: "The amount of alcohol eliminated per day does not increase with the continuance of the alcohol diet; therefore all the alcohol consumed daily must, of necessity, be disposed of daily; and as it certainly is not eliminated within that time, it must be destroyed in the system. The elimination of alcohol, following the ingestion of a dose or doses of alcohol, ceases in from nine to twenty-four hours after the last dose has been taken. The amount of alcohol eliminated, in both breath and urine, is a minute fraction only of the amount of alcohol taken."

Dr. Dupré confirms M. Lieben's observation that a substance exists in the urine of man, and in the urine of various animals, which is not alcohol, though it yields iodoform. The author found, that after six weeks of total abstinence, and even in the case of a teetotaler, that this substance, the precise nature of which has not been determined, is eliminated in the urine, and perhaps also in the breath. The quantity met with in the urine is very small, and it was found

that after the elimination due to the administration of alcohol had ceased, the amount of the substance eliminated in a given time at the first remained below the quantity normally excreted, and only gradually rose again to the normal standard. The presence of this body in urine must throw great doubt on many of the previous determinations of alcohol in urine. It passes over with the first portions of the distillate, it yields acetic acid on oxidation, and gives the green reaction with bichromate of potash and sulphuric acid, yields iodoform, and its aqueous solution has a lower specific gravity and a higher vapour tension than pure water.—*Med. Times and Gazette*, Feb. 17, 1872.

MEDICAL PATHOLOGY AND THERAPEUTICS, AND PRACTICAL MEDICINE.

15. *Lactic-Acid Theory of Acute Rheumatism.*—Dr. BALTHAZAR W. FOSTER relates (*Brit. Med. Journal*, Dec. 23, 1871) two cases of diabetes, for the cure of which lactic acid was given, and in both the medicine caused an attack of acute rheumatism.

Mr. F. remarks, "the above record contains an account of the joint-symptoms which were observed in two cases to follow the administration of lactic acid. In the first case, at least six well-marked arthritic attacks occurred; in the second case, under conditions less favourable for observation as to duration of treatment and place, one well-marked attack occurred. The phenomena corresponded in all respects to those which are characteristic of acute articular rheumatism. They came on when the acid was taken, and ceased when it was discontinued. When moderate quantities of the acid were tolerated, an increase in the dose was succeeded by the painful inflammation of the joints. Coinciding with the development of the articular affection was the appearance of perspiration, at first only slight, but afterwards, in the more severe attacks, copious and acid.

"These facts have dispelled the last lingering doubt in my mind as to the truth of the lactic-acid theory of rheumatism. At first I doubted the connection between the administration of the acid and the production of the rheumatic phenomena. In my scepticism, I regarded it as an accidental combination. The recurrence of the joint-symptoms, however, on March 13th, following distinctly on the repetition of the lactic-acid mixture, shook my disbelief. The coincidence of joint-attacks with the use of the drug might occur once, and I thought even a second time; but, when I found it occur over and over again, there was no room left for the hypothesis of coincidence. To refer Wright's attacks to a series of accidental combinations requires, in my opinion, a much livelier faith than to accept the lactic-acid theory of acute rheumatism. If, to some, Wright's case presents not evidence enough in the beautifully typical character of the artificially produced disease, and in the precision with which it could be manufactured at the will of the experimenter, then the second case comes in to refute any explanation founded on the assumption of an idiosyncrasy on the part of one patient.

"In health, no doubt, much larger quantities of lactic acid than any given in my cases would be excreted without producing any perceptible disturbance in the bodily functions. The acid would escape by the skin, the kidneys, or, after oxidization, as carbonic acid and water. It cannot be justly argued that the quantities of acid taken by my patients were too small not to have escaped in this way. The conditions under which the drug was given must be borne in mind. In diabetes we have a state of suboxidization very unfavourable to the conversion by oxidation of new compounds; and in Wright's case this was aggravated by the serious pulmonary complications. Associated with these, there was a dry and branny state of the skin highly unfavourable to the elimination of the lactic acid by one of the common channels. Lastly, the well-known persistent acidity of the urine in diabetes points to a pre-existing

hyperacidity of the fluids. These considerations are, I think, important, as defining the conditions under which the experiments were made—conditions most favourable to the development of the specific effects of the lactic acid. It was the combination of all these which rendered Wright so susceptible to the action of the drug. By the absence of one of them (the lung-complication), and the minor degree of glycosuria, we may probably explain the slighter susceptibility in the second case. The larger doses of acid which Wright was able to take occasionally, towards the close of his stay in the hospital, find an explanation partly in his more careful management of the remedy, partly in an acquired toleration of it, and partly in the great improvement which occurred under treatment in the state of the respiratory organs and in the sugar-excretions. * * *

"In this communication, my object has been to lay before the profession facts which have an important bearing on the origin of a common and serious malady. If, by pointing out the nature of the poison of acute rheumatism, they help in the smallest degree to improve therapeutics, they will not have been observed in vain."

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16. *Cause of Diabetes.*—A most interesting and important addition to our knowledge of the cause of diabetes has been recently made by Professor Cyon, in a paper which he, along with M. Aladoff, has communicated to the Imperial Academy of Sciences at St. Petersburg, and which is published in the *Mélanges Biologiques*. Several years ago, Claude Bernard put forward the theory that in diabetes the functional activity of the liver is increased, and a larger amount of sugar formed in it in consequence of the vessels of the organ becoming dilated, and the circulation in them more active. The increased activity of the circulation he attributed to some change in the vaso-motor system of the hepatic vessels, which allowed their walls to relax in a similar way to those of the ear of the rabbit after the sympathetic has been divided in the neck. He ascertained that the formation of sugar could be greatly increased, and diabetes produced, by galvanizing the pneumogastric nerves in the neck, or by irritating their roots by puncturing them at their origin in the fourth ventricle. At the same time that the production of the sugar was increased by the puncture, the vessels of the liver became much dilated. It might thus have been supposed that the nervous influence which originated in the medulla oblongata and caused diabetes, passed down to the liver through the vagi. This, however, was not the case; for, when these nerves were cut and their ends galvanized, diabetes was only produced by irritation of the central end, but not by irritation of the peripheral extremity. Diabetes could also be induced by puncturing the fourth ventricle, just as readily after the vagi were cut as when they were intact. If the splanchnic nerves were cut before the fourth ventricle was punctured, no diabetes was produced; but if they were cut after the puncture had been made, their section did not remove the diabetes which was present.

From these and other experiments, Bernard concluded that the air inhaled during respiration irritated the ends of the vagus in the lung; that this irritation was conducted up to the medulla oblongata, and was thence reflected down the splanchnic nerves to the liver, and caused the formation of sugar. By what way the nervous influence passed from the medulla to the splanchnics, however, was not ascertained; and no very satisfactory explanation could be given of the fact that section of the splanchnics after puncture of the ventricle did not remove the diabetes. Some light was thrown upon this question by the observations of Eckhardt and Pavy, but it has been reserved for Cyon completely to solve it.

Pavy noticed that section of the superior cervical ganglion of the sympathetic might cause diabetes; and Eckhardt found that it followed section of the last cervical or any thoracic ganglion, just as certainly as puncture of the fourth ventricle. Section of the splanchnic nerves did not produce it.

Eckhardt tries to explain this difference between the effect of dividing the ganglia and the nerves by supposing that diabetes is due to irritation of the ganglia by the exposure of their cut surfaces to the air, and that, when the

nerves are divided between the ganglia, paralysis and not irritation is produced. If this hypothesis were true, diabetes should not occur when the ganglia are completely extirpated; but Eckhardt does not say whether it does or not, and therefore Cyon determined to perform this operation, and thus test the truth of Eckhardt's theory. When he cut through the last cervical or the first dorsal ganglion, he found, like Eckhardt, that diabetes was produced; but it occurred just as certainly when both ganglia, or even the last cervical alone, were cut completely away, or when the nerve-fibres entering this ganglion were all cut through, although the ganglion itself was never touched. This clearly showed that Eckhardt was wrong, and that the diabetes occurring after operations on the last cervical or first thoracic ganglia was due to paralysis of the nerves which were connected with them, and not to irritation of the ganglia themselves. He next ascertained that all the fibres entering these ganglia had not the same effect in causing diabetes, for it occurred when either of the branches which pass along the vertebral artery from the spiral cord to the last cervical ganglion, or the two fibres which connect it with the first thoracic ganglion, were divided, whereas section of the other nerves proceeding from the ganglion did not do so. It thus became evident that the nerve-fibres whose paralysis causes diabetes, come from the spinal cord through the vertebral nerves to the last cervical ganglion, and pass from it to the first dorsal in the two connecting branches which, in their course from one ganglion to another, inclose the subclavian artery, and form the annulus of Vieussens. So much having been ascertained, it would seem easy enough to trace the nervous path down the gangliated cord and splanchnics to the liver; and one would expect that, by dividing the cord in the thorax, and thus paralyzing the fibres going to the liver, diabetes would be produced as certainly as when they were divided at the level of the vertebral artery.

Such, however, was not the case; for not only did subcutaneous division of the gangliated cord between the tenth and twelfth ribs not produce diabetes, but, if the cord were cut before, or at the same time as the last cervical or first thoracic ganglion, the diabetes which would otherwise have occurred did not appear. But when diabetes was first produced, division of the cord did not diminish it, or even hinder its increase, just as Bernard had found with regard to the splanchnics. In order to explain this apparently contradictory result, Cyon set about investigating the way in which the fibres of the annulus of Vieussens affect the liver, and more especially the circulation in it.

On irritating these fibres, he found that a number of fine white lines appeared round the lobules of the liver, in the position occupied by the small branches of the portal vein and hepatic artery; and these were so numerous as to produce the appearance of whitish spots on the organ, which continued while the irritation lasted, and disappeared after it ceased. At the same time that these spots appeared, he noticed that any cut or tear in the liver bled less freely than before. This indicated that contraction of the portal vein or hepatic artery, or of both, had been occasioned by irritation of the annulus; but, in order to make assurance doubly sure, as well as to find out whether it was the artery or the vein that contracted, he put a T-canula into the hepatic artery, and connected it with a manometer. On then irritating the annulus of Vieussens, the pressure rose in the manometer as much as thirty to seventy *millimètres* of mercury; while in the carotid it only rose five to ten *millimètres*. To remove the last objection which might be raised, and show conclusively that the rise of pressure was due to contraction of the branches of the hepatic artery in the liver, and not to any other cause, he compressed the artery beyond the point where the canula had been inserted, so that no change in the calibre of its branches could have any influence on the blood-pressure in its trunk. On again irritating the annulus, he found that no alteration in the pressure was produced. Division of both annuli produced, as was to be expected, dilatation of the branches of the hepatic artery, and fall of the blood pressure in it. When the portal vein was experimented on in the same manner, the pressure only rose ten or twelve *millimètres* during irritation; and he thinks this is probably due indirectly to the change in pressure in the artery.

These experiments completely prove that the vaso-motor nerves of the

hepatic artery are contained in the annulus of Vieussens; that their division causes the vessel to dilate, and at the same time produces diabetes. The theory of Bernard, that the diabetes depends on the dilatation, and on the consequent rapid circulation of blood in the liver, is thus rendered in the highest degree probable. But why should section of the splanchnics or of the gangliated cord prevent the production of diabetes, but not remove it when present? This Cyon also explains. These parts of the nervous system contain the vaso-motor fibres for the vessels of the intestines; and, when they are cut, the vessels dilate, and blood accumulates in them to such an enormous extent that there is either too little blood remaining, or it is under too low a pressure for the circulation in the liver to become increased above its normal even although its vessels be dilated. When the hepatic vessels, however, are dilated first, the blood continues to pass through them, and diabetes continues, even although the intestinal vessels have become relaxed.

The researches of Cyon, along with those of Bernard, render our knowledge of the part which the nervous system plays in influencing the production of sugar in the liver, and in causing diabetes, in so far as this disease depends on increased formation and not on diminished combustion, tolerably clear, though still incomplete; and enables us to form some kind of idea of the manner in which opium and allied remedies prove beneficial. The irritation which the inspired air produces on the ends of the vagi in the lungs is conveyed up these nerves to the medulla oblongata, and there exerts an inhibitory action on the vaso-motor nerves of the liver. When the irritation is increased, as by galvanizing the vagi, the inhibitory action is so great as to produce complete paralysis of the vaso-motor nerves, and induce diabetes; and, on the other hand, when the vagi are cut, the vaso-motor nerves act more powerfully, causing the vessels of the liver to contract, and the production of sugar to diminish, as Bernard found that it did. It seems, therefore, not improbable, that the beneficial action of opium and its alkaloids is due to their lessening the excitability of the vagus. We can hardly suppose, however, that diabetes is not sometimes due, either in whole or in part, to diminished combustion; and the causes of this still remain a matter for future investigation.—*Brit. Med. Journal*, Dec. 23, 1871.

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17 *Pathology of Scarlatina, and the Relation between Enteric and Scarlet Fever.*—This is the title of a paper read by Dr. John Harley before the Royal Medical and Chirurgical Society, December 12th, 1871. In the first portion of the communication, the author treated of the morbid anatomy of scarlatina, and gave the details of twenty-eight cases of his own observation. Of these, the greater number died on days ranging consecutively from the third to the fifteenth; the remainder died on the seventeenth, twentieth, twenty-fourth, twenty-ninth, thirty-third, forty-first, and sixty-ninth days. More or less albuminoid and fatty degeneration of the kidneys existed in six of the cases, and these died on the fifteenth, seventeenth, twentieth, twenty-ninth, forty-first, and sixty-ninth days respectively; the kidneys were healthy in the remainder. The pathological changes common (with a few exceptions depending chiefly upon the time of death) to all the cases were as follows:—

1. *The Formation of Fibrinous Clots in the Heart and Great Vessels during a Pyrexial Condition at any Period of the Disease.*—This was the commonest cause of death during the early stage. It was indicated in life by the reduction, often very sudden, of a full and bounding pulse of 120 to a dribble of 150 or 160 almost imperceptible impulses; and the failure of the heart's action was commonly attended with orthopnœa and delirium, from obstruction of the pulmonary and cerebral circulation. On opening the body before it had lost a degree of temperature, and while the blood was hot and fluid, the right heart would be found distended partly with dark blood, which coagulated on exposure; and partly, sometimes chiefly, with a large, firm, white bifid clot, continuous through the auriculo-ventricular opening. Each portion was interlaced with and firmly adherent to the tendinous cords and muscular bands of the cavity in which it lay; and each portion sent a rope-like prolongation into the orifice of the great vessel connected with the cavity. These processes fre-

quently were prolonged, in ramifications corresponding to those of the blood-vessel, into the cranial cavity, and into the lungs. These partial casts of the great vessels were often nine inches long, and occupied vessels of the sixth and seventh degrees of ramification.

2. *Marked Derangement of the Hepatic Function.*—The bile was examined in twenty cases. In five only was the secretion normal, and in these cases death occurred on the third, fourth, twenty-fourth, forty-first, and the sixty-ninth days respectively. In the remaining fifteen cases the bile was deteriorated. In two the coats of the gall-bladder were injected, and the mucous membrane rose-coloured. In the first of these cases there was a complete absence of bile, there being only a few drops of colourless alkaline fluid. In thirteen other cases the bile was greatly deficient in solid matters. The specific gravity did not exceed 1014, and the amount of solid matter in 1000 grains in no case amounted to more than 36.4 grains. In one case there were only 11.1 grains of solid matter in 1000 grains. In the majority of the cases the bile was turbid from epithelial *débris*, but on standing it became transparent, and resembled pale urine. In all the thirteen cases there was a notable deficiency of biliary acids, and in two a complete absence. The colouring matter of the bile was present in every case. If, as rarely happened, the bowel contained solid feces, it was of a pale ochre or sulphur colour. But the fecal matters were commonly fluid, grumous, or flocculent, often slimy, and of a pale ochre colour. Such, also, were the characters of the stools before death in many cases.

3. *General inflammation of the lymphatic glands* was present, usually confined to those of the neck, but occasionally extending to those of the extremities; of the spleen and mesenteric glands; and of the whole of the solitary and agminated glands of the alimentary canal, but commonly affecting only those of the fauces, and of the ileum and colon. These morbid appearances were remarkably uniform, and were observed in every case. The tonsils and solitary glandulæ of the tongue, and the external glands of the neck, were perceptibly affected in every case. In several cases large buboes formed in the neck; in three these were associated with diffuse cellulitis and purulent infiltration; and in one of these the popliteal and axillary glands and their surrounding connective tissue were similarly affected. In these cases the glands themselves were slow to take on suppurative action; and, although they were generally much enlarged and purple, comparatively few had softened centres. The spleen was enlarged in twenty-three cases, and in five of these it was increased to nearly twice its ordinary bulk. In two others it was not examined; and in the remaining two cases it was of the normal size. The mesenteric glands were swollen and inflamed in every case. In eight cases the mesentery formed a thickened, heavy, lobulated mass, and many of the glands were as large as walnuts or pigeons' eggs. Even the small glands in the attached borders of the transverse and descending mesocolon were purple and turgid. The solitary glandulæ of the ileum were in a condition of psorentery—*i. e.*, forming white granular and more or less hard elevations, like a thick sprinkling of large sago-grains upon the mucous membrane—in fourteen cases. In six other cases, the solitary glandulæ were only partially affected, the swelling was more diffuse, the glandulæ being only moderately raised; but they were always deeply injected, and in some cases had an abraded appearance. In three cases, in which death occurred on the eleventh, seventeenth, and sixty-ninth days of the fever, there was only very slight swelling of a few of these glands; and in four cases, in which death occurred on the fifteenth, twenty-fourth, twenty-ninth, and thirty-third days respectively, the glandulæ were altogether unaffected. The agminated glands of the ileum were more or less swollen and inflamed in every case but one—that in which death happened on the thirty-third day. In one case (death on the seventeenth day from suppurating buboes in the neck), there was only very slight swelling. In all the other cases the results of inflammatory action were decided, and in many cases severe. The glands were commonly raised the eighth of an inch above the surrounding mucous membrane, than which they were always more deeply injected. In the greater number of cases the mucous membrane was generally pale, and sometimes thin and bare, while the agminated glands were of a vivid red or claret colour. The inflam-

matory action was usually confined to the glands in the lower third of the ileum, but in three or four cases the whole of the patches from the jejunum downwards were affected. In some of the larger glands isolated foci of inflammatory action were occasionally observed. The interfollicular ridges were often the eighth of an inch wide, giving to the paler glands a spongy appearance; but these ridges were as often vascular, with fine hair-like turgid vessels, and in some cases they were prolonged into folds a quarter of an inch in length. In two or three cases an almost bleeding gland had a softened, abraded surface. The mucous membrane of the ileum was itself severely inflamed in two or three cases, and was covered by a thick, adherent layer of white, opaque mucus. The solitary glandulæ of the large intestine were enlarged and inflamed in eight cases. In one of these there was acute desquamation of the mucous membrane of nearly the whole of the bowel. In another case the cæcum was severely congested. In those of the remaining cases in which the large intestine was examined, it was quite healthy.

On taking the above described pathological conditions into one general view, it appeared that there was an increase of fibrin in the blood during an attack of scarlatina, and that death was likely to occur during the first week from its deposition in the heart and great vessels; that the condition of the biliary function was such as to lead to an outbreak of diarrhœa, if this had not already happened; that a latent enteritis, sometimes general, but commonly only affecting the solitary and agminated glands, existed in a high state of development during the pyrexial stage of scarlatina, ready to declare itself openly upon very slight provocation; that this intestinal affection was only a part of a general lymphatic inflammation involving the whole of the lymphatic system, including the mesenteric glands and the spleen in one common action; and, further, that this condition might persist in some degree, either in the bowel or the mesentery, as late as the sixty-ninth day, and without any outward indication of its presence throughout.

From this view one general conclusion was inevitable—viz., that the pathological changes accompanying an ordinary attack of scarlatina included all those of the first stage of enteric fever, and that the transition from one disease to the other was but a natural pathological sequence, readily determined by any cause which might increase the intestinal irritation.

The proofs of this interchange, or sequence, constituted the second part of the subject, and contained accounts of several original observations.—*Brit. Med. Journal*, Dec. 23, 1871.

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18. *Unilateral Atrophy of Tongue.*—Mr. Wm. Fairlie Clarke communicated the following interesting case of this to the Royal Medical and Chirurgical Society, November 28, 1871: Mrs. H., aged 45, had a malignant tumour removed from her right breast on February 16, 1870. The wound healed slowly, but satisfactorily. On April 15 she complained of cough and slight dyspnœa, the latter only noticeable after exercise. Under treatment, the cough soon disappeared, but the dyspnœa still continued. On October 3, Mr. Hume, of Islington, was called suddenly, and found her suffering from a deep-seated pain on the right side of the head, of periodic character, returning each night between 1 and 2 o'clock A.M., and rendering her for some hours incoherent and unmanageable. The only thing which gave her relief was morphia in grain doses. At this date, atrophy of the tongue was first noticed, though it was not then so marked as it afterwards became. On March 29, 1871, Mr. Hume was again urgently required to attend, and found the patient suffering from all the old symptoms in an aggravated degree; she had also alarming dysphagia, with paroxysms of suffocation, which recurred about three times in the twenty-four hours. On being asked to protrude the tongue, she always appeared unable to do so at first; and, after a few moments, put it out very slowly. The tongue was puckered and crimped along its whole right side from base to apex, mostly at the anterior two-thirds. An actual loss of substance had taken place, but it was bounded exactly by the median raphé; and the contrast between the plumpness of the left side and the shrivelled aspect of the right was very striking. When it was protruded, there was no

deviation to either side. Articulation was slow and difficult. There was great pain along the right side of the neck, with a certain fulness and turgescence of the vessels; but no tumour could be felt in any part of the mouth or neck. Together with these symptoms, there were general cachexia and great prostration. From this time the dysphagia and dyspnœa gradually became worse; and on June 7, in one of the attacks of suffocative cough, the patient died. At no time during her illness had there been any paralysis of the extremities, and her intellectual faculties remained clear throughout.

Unfortunately, no necropsy could be obtained; but, looking at all the circumstances of the case, Mr. Clarke thought there was good reason to believe that the ninth nerve on the right side was involved in a secondary cancerous tumour, such tumour being situated either within the cranium or at the upper part of the neck, and pressing upon the right hypoglossal nerve, and more or less implicating the pneumogastric and glosso-pharyngeal nerves as well.

The writer then proceeded to compare with this case two other instances of well-marked unilateral atrophy of the tongue; the one related by Dupuytren in the *Leçons Orales* (lecture on hydatid tumours); the other by Sir James Paget, in the third volume of the *Transactions of the Clinical Society*. The experience of Romberg and of Bidder was adduced to show that this remarkable condition of the tongue might be produced by a lesion of the ninth nerve; and to establish the same point the author related an experiment that he had made. On October 25, he divided the right hypoglossal nerve in a rabbit, and took out a piece about a quarter of an inch in length. Immediately after the operation, and during the whole time that the animal was under observation, the tongue was strongly protruded to the right side. On November 27 the rabbit was killed. It was found that the nerve had united by a soft, gelatinous, and highly vascular substance, of about twice the ordinary calibre of the nerve. The right side of the tongue, along its posterior half, was slightly wasted and flattened.

The preparation was exhibited; and an outline sketch, illustrating the case which had been related, also accompanied the paper.

Dr. John Harley agreed that the lesion of the tongue in Mr. Clarke's case was probably due to cancer pressing on the ninth nerve. But lateral deviation of the tongue also took place in some cases of scarlet fever, where the deeply-seated glands of the neck were enlarged and pressed on the hypoglossal nerve. He had met with this in a lad aged 15, who had also occipital pain and vomiting; the tongue deviating to the right when protruded. Dr. Charlton Bastian thought it by no means certain that atrophy of the tongue was always produced by paralysis of the hypoglossal nerve. At the Hospital for Paralysis and Epilepsy, he had seen a case where the only nerve that could be found affected was the sixth; the tongue was much wasted, but the deviation was very slight; and when the wasted half was tested by faradization, decided contraction took place. Dumesnil had a case of general muscular atrophy with paralysis of the tongue and face on one side; there was complete paralysis of the hypoglossal nerve, but no wasting of the tongue. The anterior roots of many of the spinal nerves were much atrophied; as also were the roots of the facial and hypoglossal nerves, while those of the fifth pair were healthy. Dr. Jaccoud, in referring to this case, explained it by supposing that the sympathetic or trophic nerve-fibres went to the tongue with the branches of the fifth rather than with those of the motor nerves. Dr. Habershon referred to the case of a woman in Guy's Hospital in whom, while she was suffering from cancer of the breast, the right side of the tongue became flaccid and wrinkled, the organ being turned to the right. Speech and the sensation of the organ were unimpaired. The right sterno-hyoid muscle was paralyzed; and the posterior belly of the omo-hyoid contracted on each side. The patient had pain at the back of the head, and pain and tenderness down the spine. After death (which took place from wasting), numerous soft cancerous tubera were found implicating the bones of the skull and the dura mater; and one of them crept up from the foramen magnum to the internal auditory meatus, involving the eighth and ninth nerves. The proximity of the disease to the meatus explained a slight affection of the facial nerve which had been noticed during life. The affection of the tongue was evidently due to the interference with its nerves by the me-

dullary cancerous growths. Dr. Hughlings Jackson thought Mr. Clarke's case a very rare one. In no instance had he seen palsy of the tongue, either on one side or on both sides, without palsies of other parts. Thus, in a case of syphilitic disease there was palsy of the left portio dura and eighth nerves, as well as of the left ninth nerve. In a case of a tumour of the medulla oblongata and pons Varolii, there was palsy of the fifth, sixth, seventh, and eighth nerves, as well as of the ninth, on the left side, and paralysis of the right arm and leg. He mentioned a case of a man who found out one morning that he was hoarse, and that his tongue was turned to one side "like a hook." There was palsy, with wasting, of the right side of the tongue, paresis of the right side of the palate, and palsy of the right vocal cord. As the man was past fifty years of age, as he had albuminuria, and as the symptoms came on in one night, the probability was that they were the result of a clot. In one case of sudden palsy of the tongue, palate, and orbicularis oris which he had seen, Dr. Lockhart Clarke discovered relics of effusion of blood in the medulla. Dr. Lockhart Clarke's researches, showing the close relation of the lingual and spinal accessory nuclei, gave the explanation of cases of lesions in the medulla producing palsies of the several factors concerned in articulation, deglutition, and voice. Dr. Hughlings Jackson had never seen wasting of the tongue from paralysis of the fifth nerve, although the temporal and masseter muscles wasted. Dr. William Ogle referred to a case related by Dr. Hyde Salter in his article on the tongue in the *Cyclopædia of Anatomy*, in which paralysis and atrophy of the tongue were produced by a wound of the neck injuring the hypoglossal nerve. He had often divided the hypoglossal nerve in animals, and had found that the tongue did not always deviate towards the side of the lesion; sometimes it was protruded straight, sometimes it was even turned a little to the opposite side. This last occurrence was difficult to explain; it might occur in cases of disease of brain, where the lesion was double; or possibly the observation might be made when the tongue was partially withdrawn after protrusion, and when, from a reversal of the muscular action, it would appear somewhat pushed to the other side. Section of the hypoglossal nerve caused the tongue to be as it were turned up, the paralyzed side being the higher. He asked if this had been noticed in disease. Dr. Hilton Fagge had seen a case of unilateral atrophy of the tongue in a boy aged 5½ years. A piece of the odontoid process was found sticking up through the dura mater. Mr. Thomas Smith thought the action of the genio-hyo-glossus muscle sufficient to draw the tongue over to the paralyzed side. This muscle protruded the organ; which, when outside the mouth, was moved by the stylo-glossus muscle; and this, or the lingualis, might draw the tongue towards the sound side. Mr. Henry Power said that the theory of the existence of trophic nerves was almost entirely based on observations of the effect of injury of the fifth nerve on the eye. He thought, however, that little if anything was known regarding trophic nerves. In Mr. Clarke's case, he thought the lesion was not connected with the fifth nerve, but with the hypoglossal. In the case of the eye, he believed that the increased liability to inflammation after injury of the fifth nerve arose from the organ being more exposed to injurious influences acting from without. Mr. Soelberg Wells referred to some experiments of Meissner, in which sloughing of the cornea did not occur after experiments on the fifth nerve unless the innermost fibres were divided.—*Brit. Med. Journ.*, Dec. 9, 1871.

19. *Hysteria*—Dr. Edw. J. Tilt gives (*Brit. Med. Journ.*, Dec. 16, 1871) the following as his views of hysteria: "I think it requires two factors for its production—1. A predisposing nervous state; 2. The stimulus of some determining cause.

"Of the predisposing cause, we may safely say that it must depend on that modification of the nervous system which makes the nervous system of woman more prone to emotion than her mate; otherwise, how is it that the disease is in the main feminine, and only met with in men whose nervous systems are built on the feminine type? We moreover know that, although a disease of every climate and social condition, hysteria is most frequent in women of the upper classes of the civilized races, in whom emotionalism is intensified,

at the expense of reason and self-control, by injudicious training in childhood, and the subsequent pampering that ill fits them for the trials of life. We can go no further than to say that this undue action of the brain is the predisposing cause of hysteria. It may be that, in severe cases, this predisposition may be so strong as to be of itself sufficient to bring on the disease. At all events, we know that there are various degrees of intensity in this predisposition, and that the slightest determining cause will make some women hysterical. In a family with which I am intimate there are ten healthy children, whose parents are not in the least nervous; but a paternal uncle is insane; two maternal uncles died of delirium tremens; one brother has been epileptic from childhood; and a sister died of meningitis. Out of these ten children, two little girls—one seven the other eight years old—burst into tears if they are looked at, if they are not placed as they like at table, and are not helped in their right turn. They pass rapidly from laughter to tears, which will flow for hours and very abundantly. They have sometimes globus hystericus. These symptoms have been repeatedly quelled by preparations of iron; but they occasionally return, and must be taken as evidences of the hysterical state, very likely to be followed by the worst manifestations of the disease on slight provocation.

"With regard to the determining causes of hysteria, I must first mention those that intensify all nervous affections—debilitating influences, like loss of blood, diseases, physical shocks, mental and emotional shocks, prolonged worry, and want of sleep. Neither should I omit the contagion of one hysterical nervous system on another predisposed to become so.

"Coming to the most important causes of hysteria—those originating in the viscera—I will first remark that, as with our mental acts, so with our emotions, they are conceived in the brain; and that old physiology and the poetry of all times have erred in placing the actual origin of our passions in our abdominal organs. Still universal consent shows how strongly they are acted on by emotion that, in fact, in the viscera are the reflex centres of emotion that stimulate the nervous system to emotional acts.

"If I have, therefore, been correct in ascribing hysteria to undue action of the brain as an organ of emotion, a potent cause of hysteria must be found in undue action of one or other of our viscera. It is, no doubt, wonderful that bodies shared by us with the lower animals should not only support the bodily structure, but, by their healthy action on the brain, give lucidity to the mind and warmth to the feelings, making genius more admirable and charity more godlike. This sounds like poetry, but becomes plain matter of fact when we remember how often anger has caused jaundice, and how frequently a host of distressing mental and emotional sensations are due to that state of liver and stomach derangement which we call biliousness, and which doubtless acts by deranging the functions of the neighbouring great ganglia. I have likewise seen repeated attacks of hysteria brought on by biliousness, and their recurrence prevented by such measures as are best calculated to prevent biliary derangement. Such cases are, however, very rare, when compared with those in which the determining cause of hysteria is an ovarian or uterine ailment. The statistics of Landouzy, Brierre de Boismont, and Dubois d'Amiens, as well as the recent assertions of Dr. Crichton Browne, show this to be the case; and those who deny it must bring forward similar masses of equally well-digested facts.

"What, then, are the diseases of the sexual system that cause hysteria? Not those in which the structure of the ovary and womb are almost destroyed—acutely, as in abscess of the ovary, slowly, as in ovarian tumours and uterine cancer—but, as a rule, the mildest forms of anæmic ovarian uterine disease; showing that it is not the intensity of the disease that causes hysteria, but the fact of its coincidence with a nervous system prone to become hysterical. Thus, hysteria is most frequently caused by those limited ovarian lesions that I have described as subacute ovaritis, lesions depending on morbid ovulation, and that frequently pass unrecognized under the disguise of diseases of menstruation. Of uterine affections, it is chiefly the milder sort—that are mucous membrane deep—which cause hysteria; and sometimes, by applying nitrate of silver to an ulcerated cervix, we most unwittingly bring on an attack of hysteria, in

patients who presented no signs of its being likely to come on, and thus experimentally prove that the two complaints may stand in relation as cause and effect. On one occasion, I thus brought on an attack in a lady, who had never before had one.

"How is the brain, laden with emotion, to be brought into contact with the viscera, the reflex centres of our emotions? The late Dr. Todd thought that hysterical delirium and other hysterical phenomena might be explained by toxæmia resulting from retained menstrual blood: but hysterical phenomena frequently arise before there is any menstrual blood to be retained; and Dr. Handfield Jones agrees with me, that with hysteria, as with other neuroses, there is no blood-poisoning. The distance between the brain and the viscera, between mind and appetite, is bridged by the ganglionic nervous system, which unites the viscera by a federal bond of union, and places this federation in intimate connection with the cerebro-spinal system. When the ganglionic nerves transmit healthy impressions to the brain, they pass unnoticed; but a hysterical fit shows how differently nerves and ganglia act when visceral action is more or less diseased.

"In many hysterical fits, after a period of incubation, in which the system seems to become more and more charged with excitement, the attack begins by pain in the womb and ovaries. Soon the hysterical aura passes to the epigastric ganglia, and, concentrating there, gives rise to the suffocation and distress characteristic of the disease. Ascending still higher, the hysterical aura reaches the cervical ganglia, producing the sense of strangulation; it then attacks the brain, deranging its functions in ways too numerous to be mentioned, and, at the same time, deranging more or less the functions of the spinal cord, according to the degree of tension of the hysterical aura. For a time pain will thus concentrate—sometimes in the brain, sometimes in the visceral ganglia—and the patient collapses into prostration when the system has been sufficiently relieved by convulsions and by critical discharges. It has been possible, in cases published by Romberg and Schulzenberger, to produce the succession of phenomena just described by simply pressing on the ovaries; and I have repeatedly brought on unconsciousness in a nervous patient of mine by pressing the left ovary."

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20. *Spontaneous Hydrophobia.*—The following very interesting case of this was reported to the Belgian Academy of Medicine by M. Guillery. The subject of it was a vigorous man æt. 71, tailor, who had always had the best of health, was attacked on February 5, 1871, with pain in the head, neck, and temporal region, which he attributed to cold. He thought little of it until he found, in the evening, on placing his hands in water for the purpose of washing them, that he was seized with a violent and painful spasm of the throat, and a great repugnance to repeat the immersion. Feeling thirsty, he tried to drink some water, tea, and coffee, but at each attempt the spasm returned and prevented him. This state of things continuing, M. Guillery was called to him on the 7th, and found him quite calm, speaking of his malady more as a curiosity than a suffering. He swallowed some meat without difficulty, but dared not appease his ardent thirst. Introducing fluid into the mouth without seeing it, by means of a tube, had also brought on the spasms. Persuaded to try, he managed by an immense effort to get down two teaspoonfuls of coffee, but declared that he preferred enduring the thirst to repeating the painful attempt. The mere sight of liquid brought on the spasms, accompanied by paroxysms of intense and prolonged fear. The liquid once removed, he conversed calmly upon his strange malady. His pain of the head had disappeared; there was no fever or acceleration of pulse, and his tongue was only whitish. On the 8th, the symptoms described were found persisting and aggravated, cruel suffering having been produced by attempting to wash the hands. The only thing the patient could take was a few morsels of bread dipped in wine, which had to be conveyed from behind, so that he should not see them. Mere looking at a glass of water produced a fearful spasm. An hour after he died, declaring that the attempt to do this had killed him. No post-mortem was performed. It is quite certain that he had not been bitten by any dog, and from this M. G. says we must admit that hydrophobia and the "*rage*" may exist independently of each other,

and that we are authorized to conclude, 1st, that the "*rage*" alone is a disease necessarily communicated to man, and that hydrophobia is one of the symptoms more or less frequently present. 2d. That hydrophobia is a distinct neurosis, which may occur spontaneously in man, and all the cases of which hitherto observed have been speedily fatal.—*La Tribune Médicale*, Jan. 21, 1872, from *Bulletin of Belgian Academy*, No. 8, 1871.

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21. *Epidemic of Essential Jaundice.*—M. DECAISNE communicated to the French Academy of Medicine an interesting account of such an affection which prevailed last autumn in Paris and its environs. His personal experience relates to twenty-eight cases (occurring between October 15 and December 8), seventeen being men from 21 to 61 years of age, and eleven women of from 17 to 45. With the exception of five of the cases, the disease appeared in the midst of health, and without apparent cause, the icterus first affecting the sclerotica, and spreading over the body in the course of four or five days. The velum palati, in almost all the cases, was of a uniform yellow. There was no fever or diminution of appetite; no pain or tenderness whatever was experienced in the hypochondrium. Under the use of mild aperients, or even mere expectation, the affection passed away in nine or ten days. In five of the cases there was much pain in the loins, itching all over the surface, desire to vomit, and obstinate constipation—symptoms which soon yielded to mild purgatives and abstinence. The persons attacked pursued different occupations, and were placed under different hygienic conditions. Many soldiers, also, of the regiments encamped around Paris, the sanitary condition of which was excellent, also suffered in just the same way. They applied for advice, in fact, not from feeling ill, but on account of their yellow colour.—*Med. Times and Gaz.*, Feb. 3, 1872.

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22. *The Biliary Acids in Icterus.*—Dr. E. A. GOLOWIN has found that in a few cases of jaundice, the biliary acids were absent in the urine, and in one case, to which attention is more particularly directed, the jaundice was caused by obstruction of the bile-ducts by gallstones. Some authors have tried to account for the absence of biliary acids in cases of jaundice in which biliary pigment was abundantly present in the urine, by supposing that the pigment resulted directly from changes in the blood-colouring matter, and was not at all referable to the liver, the name hematogenous icterus being invented to indicate this hypothesis. The occurrence of the case of jaundice, produced by obstruction of the bile-ducts, and in which there was no biliary acids in the urine, suggested that the simple long retention of bile might act on the hepatic cells, so as to incapacitate them for forming biliary acids, while the pigment was formed as usual, and it was thought that some of the supposed cases of hematogenous icterus might be in this way explained. With this view the author proceeded to perform certain experiments on animals, in order to determine whether retention of bile has the effect after a time of preventing the formation of the biliary acids. A biliary fistula was first made in a dog, and this was some time afterwards ligatured, so that obstruction was produced. A short time after this latter proceeding, it was found that though the bile-pigment was abundantly present, the biliary acids were not. So that this experiment supports the idea mentioned above. The author supposes that other causes besides prolonged retention of bile may cause the non-secretion of the biliary acids, and that there is no need to invent a hematogenous theory to account for the phenomenon.—*Glasgow Med. Journ.*, Feb. 1872, from *Virchow's Archiv*, vol. liii. pt. iv.

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23. *Neuralgia of all three Branches of the Fifth Nerve excited by Syphilis.*—Dr. ANSTIE read to the Clinical Society of London, Nov. 24, the further and concluding history of a case of which the earliest notes were read last session. It was an example of neuralgia of all three branches of the fifth nerve, immediately excited by constitutional syphilitic infection, and which was of recent date. The case was one of a remarkable character. The nerve had been *predisposed* to neuralgic pain; many years before the

syphilitic infection it had been the seat of an ordinary typical *migraine*, of great severity; and at present it was very noteworthy that the painful and tender points were distributed, not according to the type of tertiary syphilis, but according to that of ordinary neuralgia. Moreover, a number of secondary lesions (unilateral facial anæsthesia, unilateral loss of taste in the tongue, unilateral spasm of muscles, etc.) were distributed exactly as such secondary affections were in severe neuralgias where there was no question of syphilis. Besides these curious phenomena, there were a series of paralyses of the ocular muscles, quite of the ordinary syphilitic type. Thirty grains of iodide of potassium daily completely cured the neuralgia, the anæsthesia, the loss of smell and taste, and the muscular spasms, in a little more than a fortnight. The ocular paralysis proved exceedingly obstinate; but the prolonged use of iodide in larger daily doses (forty-five and then sixty grains) at last completely removed it. It was a singular fact that, during the full progress of the muscles toward recovery, unmistakable symptoms of iritis made their appearance; they were checked by a short course of mercury. Such a case as this was sure to be marked, in the future, by the repeated recurrence of tertiary syphilitic nerve-lesions.—*Brit. Med. Journ.*, Dec. 9, 1871.

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24. *The Spinal Paralysis of Children; Infantile Paralysis.*—MM. H. Roger and Damaschino have just completed a series of papers on this complaint (*Gaz. Méd. de Paris*, Dec. 23, 1871), and, at the conclusion, lay down the following propositions:—"1. The alteration peculiar to infantile paralysis is a lesion of the spinal marrow, which causes the atrophy of muscles and nerves. 2. The seat of this lesion is the anterior part of the gray substance of the medulla, where softened portions of spinal substance are seen. 3. This softening is of an inflammatory nature; in fact, simple myelitis. 4. Infantile paralysis should, therefore, be called spinal paralysis of children, and be classed among the affections of the spinal marrow, as depending on myelitis."—*The Lancet*, Feb. 17, 1872.

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25. *Intestinal Invagination.*—L. Ninaus relates a case of intestinal invagination occurring in a farmer, 32 years of age, which terminated favourably, after the separation and discharge of a portion of small intestine fifty inches in length. The cause of the invagination was an abnormal fold or pouch-like projection within the calibre of the intestine, proceeding from the submucous cellular tissue. The symptoms produced by this interference with the free course of the intestinal canal occurred suddenly, subsequently to severe bodily exertion. For some months previously to their onset, the patient had been troubled with occasional colicky pains and costiveness. Fecal vomiting occurred on the fourth day of the attack, the first discharge per anum on the eighth, and the expulsion of the invaginated portion of intestine on the twenty-sixth. There still remained disturbance of digestion and occasional pain.—*Centralblatt f. d. Med. Wissenschaften*, Nov. 18, 1871, from the *Wien. Med. Presse*, No. 40, 1871.

D. F. C.

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26. *Suppuration of one-half Lobe of Cerebrum; Consciousness and Ability to Labour intact; Sudden Death.*—Dr. Schwartzenthal relates briefly the case of a man 30 years of age, a day labourer, who had suffered, for over two weeks, in the commencement of May, in the year 1871, from pain in the head, with langour and want of appetite, followed by a severe fever of an entire month's continuance. After this, apparent convalescence ensued, and the patient resumed his ordinary occupation. About four weeks after he had left the hospital, whilst engaged in an altercation, he received a blow upon the head, and instantly expired. Examination after death showed that the posterior half of the right lobe of the cerebrum was reduced to a circumscribed accumulation of pus, while the anterior half of the lobe and the entire left cerebral hemisphere were of a doughy consistence. The cerebellum was to some extent softened. Up to the period of his death the patient had continued at labour without apparently the least difficulty.—*Centralblatt f. d. Med. Wissenschaften*, 1871, No. 12, from the *Wien Med. Presse*, 1871.

D. F. C.

27. *Pathology of Pyrexia.*—Dr. CHARLES MURCHISON, in his lecture on pyrexia, sums up the pathology of this disease as follows:—

"1. A morbid condition of the blood, due to the entrance of some poisonous matter from without or generated within the body, or to some local injury or inflammation, which exercises a paralyzing influence on certain portions of the nervous system, and particularly on the sympathetic and the vagus.

"2. Increased rapidity of the heart's action is one of the earliest results.

"3. A second result is a rapid disintegration of the nitrogenous tissues into substances of a simpler chemical construction, while little or no fresh material is assimilated to compensate for the loss. Increased temperature, great muscular prostration, and loss of weight are the natural consequences.

"4. The non-elimination, from any cause, of the products of this disintegration gives rise to cerebral (typhoid) symptoms or local inflammations.

"5. The impaired nutrition of the heart itself and of the rest of the body, in conjunction with the polluted state of the blood and the nervous paralysis already referred to, induces in severe cases of fever great weakness of the cardiac contraction, and stagnation of blood in the capillaries in different parts of the body.

"Why it is that the febrile process once lighted up should ever become arrested, and in many instances after a definite duration, is a problem in medical science of which no satisfactory solution has yet been offered."—*Brit. Med. Journ.*, February 17, 1872.

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28. *Treatment of Pyrexia.*—Dr. CHARLES MURCHISON remarks (*Brit. Med. Journ.*, February 17, 1872) if the views he has laid down respecting the pathology of fever be correct, our objects in treatment ought to be as follows:—

"1. To remove, when possible, the cause on which the fever depends.

"2. To promote elimination, not merely of any morbid poison, but of the products of exaggerated metamorphosis in the blood and tissues.

"3. To reduce the temperature and the frequency of the action of the heart.

"4. To maintain the nutrition of the tissues, and stimulate the action of the heart by appropriate food and stimulants, taking care, at the same time, not to excite congestion or increase the work of the already overtasked glandular organs.

"5. To relieve dangerous and distressing symptoms.

"6. To obviate and counteract secondary complications.

"How these indications are to be fulfilled will be discussed under the head of individual febrile diseases; but a few general remarks on the treatment of fever in the abstract will not be out of place here, and may save repetition hereafter. You are not to imagine that each fever has a treatment of its own. According to the predominance of certain symptoms, the appropriate treatment will vary greatly in different cases of the same fever, while in fevers widely different in their causes it may be identical.

"1. Unfortunately, it is not often that we have it in our power to remove the cause of pyrexia; but the object is one always to be kept in view, and sometimes the main efforts of our treatment must be directed to secure it; as, for example, when pyrexia depends upon pent-up pus, an obstructed bowel, or gouty, syphilitic, or periosteal inflammation.

"2. The elimination of any morbid poison, as well as of the products of exaggerated metamorphosis, will often be promoted by the judicious employment of diaphoretics, diuretics, purgatives, and emetics. The old practice of commencing the treatment of pyrexia by giving a purgative to unload the portal circulation and promote the action of the liver, is undoubtedly a good one, and is particularly advisable in persons of robust habit, or who live too well. In mild cases of pyrexia, the only treatment necessary consists in the avoidance of any chill, and in the administration of a mild aperient, followed by frequent doses of diuretics and diaphoretics, such as the citrate of potash, or the liquor ammoniæ acetatis with spirit of nitrous ether. Elimination will also be promoted by a plentiful supply of fresh air, which will favour the escape of carbonic acid from the lungs, and by the free use of diluents, which will help to wash away through the kidneys the products of tissue-waste. In all grave

cases of fever you will remember the importance of maintaining the action of the kidneys, and of keeping a good watch on the state of the urine; noting carefully not so much its colour and the presence or absence of lithates (both of which characters will depend much on the quantity), but the quantity and the presence or absence of albumen. When the quantity becomes notably diminished, or albumen appears, advantage will often be derived from hot poultices to the loins, aperients, diaphoretics, diluents, and diuretics. But while you promote elimination, you must take care that the means for this end do not weaken too much the action of the heart; and you must remember that, in some fevers, the natural processes of elimination are excessive, and conduce to dangerous exhaustion and death.

"3. For reducing the intensity of the pyrexia, different measures have been proposed.

"*Bloodletting* was at one time universally resorted to for this object, but in this country it is now entirely discarded, because it was found to increase one of the great dangers in pyrexia, viz., failure of the heart's action. There are few accurate observations on the effects of bloodletting on the temperature of pyrexia; but we know that, when a copious bleeding from the nose or the bowel takes place in enteric fever, although the temperature may fall below the normal standard, it speedily regains its former height, or rises above it.

"*The external use of cold water* is one of the most certain means of reducing temperature in pyrexia, and in certain cases is attended with good results. The attention which this practice is now attracting will justify the following remarks. In the seventeenth century, the brothers Hahn, of Leipzig, treated fevers by the external use of cold water, but their observations were soon forgotten. Towards the end of the last century (1787), cold affusion was proposed by Dr. Currie, of Liverpool, both for arresting and mitigating fever. The patient was seated naked in an empty tub or bath, and several buckets of water, of a temperature of 40 or 50 degrees Fahrenheit, were poured from a height of one to three feet, or more, over the head and chest. He was then hastily dried, and restored to bed, and, in most cases, the operation was repeated once or twice daily. It was stated that, in many cases, if resorted to during the first three days, this treatment arrested the disease; while, in others, it reduced the pulse and temperature, relieved many of the distressing symptoms, and particularly the headache, restlessness and delirium, and conducted the disease to a safer and speedier issue. The affusions were employed at any stage of the fever; but the effects were always most salutary at an early stage. They were said to be contraindicated when the temperature of the skin, ascertained by the thermometer, was not much above the normal standard, or when, notwithstanding an elevation of temperature, the patient complained of chilliness, or suffered from severe diarrhœa or profuse sweating. The wonderful results obtained by Currie were confirmed by numerous observers in different parts of the world, whose testimony is recorded in the third edition of his work, published in 1804.[1] But in the British epidemic of 1817–19, the practice was followed by many with great perseverance, and the general result, according to Sir Robert Christison, was that in very few cases, if any, was the disease arrested by it; that although an abatement of febrile heat and restlessness occurred almost invariably, it was of short duration, and not to be made permanent by any frequency or repetition; that as much good was eventually attained by frequent cold or tepid sponging, together with cold applied to the head; and that often the cold affusion occasioned for a time after each application an intense feeling of pressure and weighty feeling in the brain, which could not be regarded without some uneasiness.[2] These statements, backed by professional and popular prejudice, account, perhaps, for the subsequent neglect of the cold-water treatment of fevers. But the observations made of late years by Brand, of Stettin, Jürgensen, of Leipzig, Liebermeister, of Basle, Ziemssen of Erlangen, and H.

[1] Medical Reports on the Effects of Water, Cold and Warm, as a Remedy in Fever. By James Currie, M.D., F.R.S. 1804.

[2] Article, "Continued Fever" (Library of Medicine, vol. i., 1840).

Weber and Wilson Fox, of London, show that, although the practice may not shorten the fever, and is often inapplicable. yet under certain circumstances it is useful not only for reducing the temperature, first of the surface and then of the interior of the body, but for relieving headache and other distressing symptoms, removing congestion of the kidneys, warding off delirium and coma, and rousing the nervous system in cases of excessive stupor. The circumstance has perhaps been too much lost sight of that cooling the body may not influence the conditions on which the development of heat depends; but with reduced heat it may be assumed that there will be diminished metamorphosis, to the non-elimination of the products of which many of the dangers of fever are due. In point of fact, Schroeder, of Dorpat, has ascertained that cold baths effect a marked diminution in the excretion of carbonic acid and urea in fever;[1] and as this was not attended by any aggravation of the general symptoms, it is fair to attribute it to a retarded metamorphosis of tissue.

"Statistics have been appealed to to prove the great success of the cold-water treatment of fever (particularly of enteric fever) as contrasted with that of an expectant method; and, although other conditions not stated may have helped to influence the result, they suffice to show that the practice is not beset with the dangers commonly imagined. But the most conclusive facts in favour of the practice are those observed in certain cases of hyperpyrexia by Dr. Wilson Fox[2] and others, where its employment was followed by recovery from an elevation of a temperature (110 deg. Fahr.) which, under every other method of treatment, has been speedily followed by death. At the same time there are many cases of pyrexia in which the cold affusion or immersion would be unsuitable or injurious. It is likely to be of most service when the temperature is unusually high; and in all cases the practice is contraindicated when the temperature is under 102 deg. Fahr., or when the extremities are cold, although the temperature of the central parts of the body be high; and it must always be employed with caution when there are signs of weakened cardiac action or of stagnation of blood in the capillary circulation, although it may be noted that in one of Dr. Fox's patients, who was apparently rescued from death, the face was cyanotic and the radial pulse imperceptible.

"There are different plans for employing cold water in the treatment of pyrexia, such as the cold affusion practised by Currie, packing in a cold wet sheet resorted to by Brand, or immersion in cold baths. The last is the method now most in fashion. The patient is placed in a bath having a temperature of from 50 deg. to 70 deg. Fahr., or better, as Ziemssen recommends, in one whose temperature is about 10 deg. below that of the body, but which, after the patient's immersion, is gradually cooled down to 68 deg. by adding cold water. He should remain in the bath for half an hour, or until shivering comes on, and all the time he is in the bath his limbs ought to be rubbed by assistants. He is then to be hastily dried and put in a warm bed. For some time after the bath, the temperature in the rectum continues to fall as the trunk parts with its heat to the extremities; but as soon as the temperature in the rectum rises again to 104 deg., the patient ought to have another bath. In the early stages of the fever, as many as seven or eight baths in the day may be necessary. When cold affusion or immersion is contraindicated or inexpedient, frequent sponging of the surface with cold or tepid water will also help to cool the body, and is often a source of much comfort to the patient."

"*Quinia in large doses* has an undoubted influence in lowering the temperature of pyrexia. In most cases of severe pyrexia, ten, fifteen, or twenty grains will, within an hour or two, cause a fall of the temperature to the extent of three or four degrees, and to a less degree of the pulse.[3] It is true that the effect passes off after a few hours, and that there is no good evidence (except in

[1] Ueber die Einwirkung kalter Bäder auf die Co^2— und Harnstoff—ausscheidung beim Typhus.—*Deutsch Archiv f. klin. Med.*, 1869, Bd. vi. s. 385.

[2] On the Treatment of Hyperpyrexia by means of the External Application of Cold. London, 1871.

[3] For evidence on this point, see Report of a Committee (of which I was a member) of the Clinical Society.—*Trans. Clin. Soc.*, 1870, vol. iii.

malarious fevers) of its cutting short the natural course of the attack; but the effect may be maintained by a repetition of the dose; and the remedy has often appeared to me to be of signal service when a pyrexia was at its crisis, and when the temperature was rising in place of falling.

"*Digitalis, aconite*, and *veratrum viride* have a marked power in reducing the pulse, and, to a less extent, the temperature in pyrexia, and are, in my opinion, too much neglected for these objects in practice. Veratrum viride is largely used in America in the treatment of fevers, and its effect upon the pulse is speedy and most decided; the only objection to its use in private practice which my experience suggests is its liability to induce sudden nausea and faintness; but these symptoms are transient, and cease on the administration of a stimulant. Ten or fifteen minims of the tincture may be given every four or six hours. Aconite is a remedy of great value for reducing the pulse and temperature in fever, and especially in the pyrexia resulting from local inflammations, and is much less used than it deserves to be. Digitalis is another remedy which I have often found very serviceable in various forms of pyrexia. While increasing the force of the cardiac contractions, it diminishes the frequency of the pulse, reduces the temperature, and increases the flow of urine. Lastly, *antimony* reduces, in a marked degree, the frequency of the pulse in pyrexia, and promotes diaphoresis and mucous secretion. It was at one time largely used in all fevers, but in many it is contraindicated by its tendency to weaken the contracting power of the heart.

"4. The nutrition of the body must be maintained by appropriate food, in the form of milk, beef-tea, eggs, and farinaceous articles. Not long ago it was the custom to starve fevers; and you may probably have heard that the late Dr. Graves, of Dublin, who was mainly instrumental in doing away with this objectionable custom, expressed a wish that his epitaph might be, "He fed fevers." The modern tendency, however, is perhaps to overfeed fevers, and especially to give too much nitrogenous food. Dr. Parkes has shown that there are theoretical objections to a purely nitrogenous diet in fevers. It is doubtful if the disintegrating nitrogenous tissues can be fed; and in that case the albuminous food must be got rid of by the already overtasked glandular organs. Milk is, in most cases, preferable to beef-tea as an article of diet in fevers.

"In many cases of fever it will be necessary to give stimulants. You must not give stimulants simply because a patient has fever. Many patients with fever do better without them. But you must not refrain from giving stimulants when the heart shows signs of weakness, as happens in the advanced stages of most protracted fevers. The heart may be artificially stimulated by sinapisms and other irritating applications to the skin, but better by the internal administration of ammonia, ethers, and alcohol, in quantities proportioned to the weakness of the heart and pulse.

"5. In every case of pyrexia, you must combat dangerous symptoms as they arise. Stagnation of blood in the pulmonary capillaries impeding the aeration of the blood is to be met by stimulants, such as alcohol, carbonate of ammonia, and ethers. Digitalis, by strengthening the heart's action, and turpentine, which seems to stimulate the capillary circulation, are also useful under these circumstances; while advantage will likewise be derived from mustard- and linseed-poultices to the chest, sometimes from dry cupping of the chest, and from warm applications to the feet. When uræmic symptoms predominate, the action of the skin and bowels is to be promoted, digitalis and saline diuretics may be given to increase the flow of urine, sinapisms and linseed-poultices are to be applied over the loins; while attempts may be made to rouse the patient by cold affusion to the head, by blistering the shaven scalp with liquor ammoniæ, and by sinapisms to the nape and feet. In many cases of fever you will also be called upon to relieve distressing symptoms—such as diarrhœa, pain, sleeplessness, and delirium—which, if unchecked, hasten exhaustion and prevent recovery.

"6. You must counteract, as far as possible, secondary complications, which will vary according to the primary cause of the pyrexia, and which always add to the patient's danger.

"Lastly, I would caution you against two errors in the treatment of pyrexia.

"1. You must take care that the remedial measures which you adopt in no way thwart the natural modes of recovery, or favour the natural modes of death.

"2. At the same time, you must not be content with adopting a treatment of pure expectancy. You must not forget that the natural termination of pyrexia may be death, as well as recovery."

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29. *Principles of Treatment of Rheumatism.*—Dr. RIDGE, after observing that an incipient attack of rheumatism can be cut short by diluents hot or cold, with abundant covering to the surface, or hot stimulants, or by a Turkish bath, proceeds to consider the means by which acute rheumatism can be removed. The inflammatory action, he thinks, may be either checked or diverted. Nerve-excited inflammation may be checked in two ways, by 1, directly reducing the nerve-energy; or 2, by locally diminishing the activity of tissue oxidation. By reducing the nerve-energy, he means a process quite distinct from its diminution in one form through diversion into another channel. He refers to a simple decline of its intensity. This may be effected by the continuous application of heat, or by the exhibition of certain sedatives, as opium, camphor, aconite, and most likely colchicum, although these can act powerfully in other ways. Nitrate of potash has a somewhat similar action on the nervous system, since it relaxes bloodvessels (so promoting catamenia) and increases the perspiration. Another mode of checking the process is by directly retarding the oxidation of the tissues. In this way probably the various vegetable salines act, for it is well known that in passing through the system these salts are decomposed and form carbonates of their base: in this process a large amount of oxygen is consumed. The same amount of oxygen is consumed by the complete oxidation of 2 grains of acetate of ammonia, 5 grains of acetate of potash, 7 grains of citrate of potash, 8 grains of acid tartrate of potash, 9 grains of tartrate of potash. When completely oxidized, however, most of these salts form alkaline carbonates, and alkalies promote oxidation: hence he thinks their secondary action will neutralize, or in some instances even reverse, their primary effect; and to the extent that they accomplish this they cannot but do harm. More powerful retarders of oxidation are met with in the acids, both vegetable and mineral, as the citric and sulphuric acids, and the hydrochloric in the form of perchloride of iron. But the morbid process may also be arrested by diverting the excess of energy into another channel, and this is the principle of the administration of all the diaphoretic, diuretic, counter-irritant, and derivative measures which are so largely employed. These agents either produce a change of action in the inflamed part itself, and substitute vaso-motor contraction or secretion, or they restore the normal trophic action by diverting the excess of energy to establish inflammation, organic muscular contraction, or secretion in some other part alternatively connected with it by its sympathetic nerve supply. The establishment or increase of some secretion is the method most frequently adopted, and to this end opium, camphor, ipecacuanha, tartar emetic, colchicum, guaiacum, alkalies, salines, and some purgatives are administered, besides the application locally or generally of warmth and moisture; secondly, we may promote vaso-motor action by means of quinia, digitalis, and ergot; thirdly, the use of counter-irritants is widely spread and their value unquestionable.—*Practitioner*, Dec. 1871, from *Med. Times and Gazette*, Nov. 4, 1871.

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30. *Turpentine in Peritonitis.*—At a recent meeting of the Paris Hospital Medical Society M. VIDAL took occasion to call the attention of his colleagues to the great value of turpentine as an external application in partial and general, and even in puerpeal peritonitis. Trousseau, originally importing this remedy from England, employed it in large doses internally. In peritonitis, M. Vidal soaks a piece of flannel thoroughly in the turpentine, and, having applied it over a large portion of the abdomen, covers it with gummed silk. It remains on until vesication is produced at several points, when the silk is removed in order to allow of the evaporation of the turpentine. Under this application he has in many instances seen patients, who were very far gone, rally completely and recover. M. Bourdon inquired whether this application of turpentine had

been employed in any cases from the commencement, and whether leeches, cataplasms, &c., had been also resorted to. In this case the turpentine would have acted just like an ordinary blister, and it is well known that in advanced peritonitis advantage is sometimes derived from resorting to blisters and Todd's mixture. He also suggested that the turpentine might act in the same way as the castor-oil collodion employed by M. R. Latour, by preventing transpiration and the contact of air. M. Vidal believes, however, that turpentine does not act in this way, but as an energetic and diffused revulsive, while at the same time it undergoes absorption by the skin and respiratory organs. At first he did not employ it so exclusively in peritonitis as he now does, as he then used to apply also leeches. Now he resorts without hesitation to the turpentine at once. He generally combines with it the application of ice, or what might be termed compression by means of ice, and under certain circumstances he would still use leeches. M. Moutard-Martin, believing the action of the turpentine to be solely topical, asked whether comparative trials of it and of blisters had been made; but M. Vidal is convinced that it also acts internally. That it is speedily absorbed is shown by the odour of the urine; while, soon after its application, the patients seem as if a cordial had been administered to them, and their cyanosed lips soon recover their colour.—*Med. Times and Gazette*, Feb. 24, 1872.

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31. *Use of the Bromide of Potassium in Epilepsy.*—Dr. Jules Falret gives the results of his treatment at Bicêtre during the years 1867–70, where he treated, almost invariably, confirmed cases where the disease was of long standing. He begins by giving 15 grains a day, very gradually increased until the dose of one drachm is reached. This usually diminishes the number of attacks, and is then persisted with; if not, the quantity is again increased, every week or fortnight, until eight scruples, or two drachms, are taken daily. If considerable improvement results, and if no bad consequences are observed, this dose is continued for one or two years, and then gradually brought down to the original dose of 15 grains. He attaches great importance to the slowness with which the dose is increased and diminished; and ascribes most failures to suddenly leaving off the medicine. By this treatment he states that he has cured one-seventh of his cases, greatly improved three-sevenths, and failed with the remainder. Attacks of maniacal violence proved most amenable to treatment; next in order came nocturnal fits, then diurnal; vertigo and the "petit mal" being most difficult of cure, these last sometimes taking the place of ordinary fits under treatment. An acneiform eruption on the face, shoulders, and back is generally observed as soon as the daily dose of one drachm is reached; when it is not produced, M. Falret usually finds that the bromide does no good—it sometimes becomes so annoying as to prevent treatment being continued. Considerable loss of mental power and other cerebral symptoms, of course, indicate an immediate suspension of the remedy. They seem to M. Falret more frequent in private practice than at Bicêtre. One patient died very suddenly, while taking only moderate doses of the bromide; the author analyzed the viscera, and found considerable accumulation of the salt in the brain and liver.

This paper was read before the Société Médico-Psychologique, and gave rise to two very interesting discussions, of which only the most salient points can be given here.

M. Morel looked upon the doses given as "fearful;" he has himself rarely given more than two scruples or one drachm daily.

M. Legrand du Saulle, on the contrary, stated that he saw no danger in such large doses, provided they were only arrived at very slowly: his full dose is two drachms and a half daily, which he only attains after several months' treatment. With this precaution, he has never seen any evil results, not even emaciation. He believes, however, that such large doses are only needed for men, and that women are sufficiently acted upon by half a drachm to one drachm daily. He has treated 138 cases in all, cured 10 of these, greatly improved 19, improved 45, and failed in 64. He prefers the bromide of sodium for chorea, hysteria, hemicrania, and the early stages of melancholia; and the bromide of ammonium

in all forms of cerebral congestion; he has seen it relieve symptoms of this kind in general paralysis with great rapidity.

M. Voison, who was the originator of this graduated method of administering the bromide, read a long critique upon M. J. Falret's paper. Besides acne simplex and indurata, he has observed another eruption of specific character in persons taking the salt. This consists of patches of confluent acne pustules, generally on the calves of the legs, which end by producing adherent scabs, and sometimes ulcers difficult to heal. He shows, by figures, that the occurrence of skin eruptions is no evidence of a favourable action of the remedy, which he looks for in some sign that it is acting on the medulla oblongata. This he finds by titillating the pharynx, nares, and epiglottis, and observing whether the reflex acts of nausea, sneezing, and coughing are produced. As soon as they cease, he considers the medicine to be producing its physiological action, and, therefore, to be acting beneficially upon the disease. He dwelt at some length on the various phenomena of bromism (among which he has once seen acute mania), and urged that no patient should be allowed to take more than one drachm of the bromide daily, without being frequently examined by his physician. Of 41 cases (generally of confirmed epilepsy) treated by M. Voison, 17 have had no attack for five years, 20 are improved, and 4 remain in the same state. No one seems to have questioned the efficacy of the bromide during these two meetings, but, since then, M. Delasiauve and a few men of less note have expressed doubts as to its curative action in confirmed epilepsy, and have seen increased violence of attacks follow its suspension. What would they have said had they known that Binz and other Germans totally deny that the bromides have any different action from the chlorides of the same bases?—*Edin. Med. Journ.*, Feb. 1872, from *Journ. Mental Science*, Jan. 1872.

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32. *Intracranial Disease cured by Iodide of Potassium.*—Dr. G. OWEN REES related to the Clinical Society of London, November 24, 1871, the following case: A young man, aged 21, was admitted into Guy's Hospital, under Dr. Moxon's care, having been ill six months. The illness came on with severe headache; in about three months, ptosis and ocular paralysis of the left side commenced, and, as it went on, the left fifth nerve also became involved, and the right hand grew partially numb. When admitted, the patient had agonizing pain in the head. The left eye was intensely red, and its cornea ulcerated; it was almost immovable, and the lid was dropped. He could not feel moderate touches on the left side of the face, nor taste salt on the left side of the tongue, nor use the left masticating muscles. He had two slight seizures of a doubtful kind on the first two days after admission. Iodide of potassium was given in three-grain doses thrice daily, and the dose increased to a scruple. The pain left him very soon, the other symptoms more gradually. He was in attendance at the Society's rooms, and the state of the left side of his face and of his left eye was practically normal again. This was the third case of syphilitic disease about the sella turcica Dr. Moxon had met with. This he connected with the growth of the sphenoidal sinuses there, bringing in illustration the occurrence of exostoses very frequently about the frontal sinuses and of exostoses on the long bones at the region of the epiphyseal cartilages; all these facts going to prove that the seats of late development were usually liable to disease. Dr. Moxon believed it was incumbent on every one who had a case of local intracranial disease under his care, to treat it at once with iodide of potassium, without waiting to make out its nature. He had not seen any serious ill effects from the iodide when taken to the extent of a drachm in a day for long periods. Slight salivation, a red rash, and catarrh were not common, though they occasionally occurred; and they were by no means to be compared with local intracranial disease as alternatives. As to absorption of the testes, he had never seen it. The iodism of old authors was probably to be referred to the poisoning of the blood by the absorption into it of broken-down matter of goitres during their cure.—*Brit. Med. Journ.*, Dec. 9, 1871.

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33. *Therapeutic Value of the Hypodermic Injection of Ergotine in Hæmoptysis.*—Dr. CURRIE RITCHIE, of Manchester, has recently employed this treat-

ment in nine cases of hæmoptysis. Case 1.—A. B., aged 22, spat florid blood in June last, after a long walk, in considerable quantity. From that time till September had more or less cough, with occasional streaks of blood in sputa, menstruation regular, was then seized, after dancing, with severe hæmoptysis, lasting from 2 A.M. till 11 A.M., when Dr. Ritchie found her anæmic, with a troublesome cough, and expectorating mouthfuls of blood. Five grains of ergotine dissolved in water were injected into the left arm, and perfect rest in bed enjoined. No expectoration of blood took place after the injection, and two months afterwards she was apparently in perfect health. Case 2.—W. S., aged 30, had mitral regurgitation, with consolidation of apex of left lung. Dr. Ritchie found him, at 2 A.M. of 20th September, labouring under profuse pulmonary hemorrhage, which had already lasted one hour. In Dr. Ritchie's presence he expectorated two ounces of florid blood. Five grains of ergotine dissolved in water were injected subcutaneously, the hemorrhage was at once arrested, and did not return. Case 3.—A man, aged 26, had purulent expectoration for more than two years, with occasional streaks of blood. On the 26th September came to Dr. R. labouring under severe hæmoptysis, which had lasted for two days continuously, in spite of medical treatment; according to his own account, he had spat up several pints of blood. His face was blanched, and the mucous lining of his lips and cheeks very pale. Five grains of ergotine dissolved in water were injected subcutaneously; not a single bloody sputum followed the injection till the 28th, when a repetition of the injection was followed by a similar abrupt cessation of the hæmoptysis, which, however, recurred on the night of the 29th, probably owing to domestic inquietude from his wife insisting on carrying him off to Ireland. Case 4.—F. B., aged 62, had suffered from slight hæmoptysis for several days. Five grains of ergotine in watery solution were injected. At the end of two days the hemorrhage continued as before; a repetition of the injection was followed by complete cessation of the hæmoptysis, only one or two small dark-brown coagula following the second injection. Case 5.—A. F., aged 17, had a cavity the size of an orange in the apex of her left lung when first seen, and shortly afterwards was attacked by severe hæmoptysis. Five grains of ergotine in watery solution were injected, and only one single bloody sputum followed the injection. No return of the hemorrhage had taken place a month after when last seen. Case 6.—J. C., aged 22, spat blood in May last for two or three days, and again a fortnight later. She remained in fair health till 11th October, when hæmoptysis again occurred to the extent of "nearly a quart of bright red blood" before Dr. R. saw her. Five grains of ergotine in watery solution were injected, and no more blood was expectorated till the 15th, when, after a severe fit of coughing, a streak of blood was detected on three several occasions in the sputum. In this case the injection produced a good deal of pain and induration round the seat of puncture, lasting till the 19th of October. Case 7.—K. M., aged 19, had slight hæmoptysis for several days; a few hours before being seen had spat up several mouthfuls of pure blood. Five grains of ergotine in watery solution were injected subcutaneously, only one bloody sputum followed, and a fortnight later there had been no return of the bleeding. Case 8.—J. W., aged 28, caught cold in March last, and continued to expectorate phlegm for two or three months; he then caught a fresh cold, and ever since his sputa have been streaked with blood. The sputa are tenacious, not frothy; expectorates most blood when at work, but feels easier there. Five grains of ergotine in watery solution injected on 1st November. Expectoration of blood took place once on 1st November, and once on the morning of the 2d; but, in the afternoon of the 2d, it became considerably increased, and continued till the 6th. So much irritation had been produced by the injection, that Dr. R. did not repeat it till the morning of the 7th, when three grains of ergotine were injected, dissolved in equal parts of glycerine and spirits of wine. There had been no return of the hæmoptysis on the 14th when last seen, and no trace of irritation from the second injection. Case 9.—Mrs. D., aged 60, under Dr. Bowman's care. On the 19th October had been spitting blood continuously for two days, except for about six hours, when there was almost complete cessation. At 3 P.M. on the 19th hæmoptysis recommenced, and she was ordered

ten-minum doses of the liquor ergotæ every two hours, without effect. At 9 P.M. she was expectorating blood profusely, an incessant hacking cough compelling her to do so every few seconds. At Dr. R.'s suggestion, Dr. Bowman injected five grains of ergotine dissolved in water, after which there was absolutely no hæmoptysis, and when last seen, on the 26th October, she was apparently quite well.—*Edin. Med. Journ.*, Feb. 1872, from *Practitioner*, Dec. 1871.

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34. *Alkaline Sulphites in Marsh Fevers.*—M. POLLI contributes a paper on this subject to the *Journal de Médecine* in which he states that his experiments on the alkaline sulphites have extended over seven years and have always been compared with quinia, and that he has arrived at the following results: 1. That marsh fever can be cured by the sulphites alone. 2. That the action of the sulphites is less rapid on the attack of the fever than the sulphate of quinia; they do not stop so suddenly the periodical course of the fever, but they usually gradually diminish the violence of the symptoms, till they disappear altogether. 3. That the sulphites, *en revanche*, act much more certainly in preventing the return of the fever than quinia. Amongst 403 cases treated by the sulphites relapses only occurred in 5.7 per cent., whilst in 183 cases treated with sulphate of quinia the relapses amounted to 44.5 per cent. 4. That many cases of miasmatic fever, long rebellious under treatment by quinia, were cured by the sulphites alone. 5. That the sulphites can be employed with success even as a prophylactic means, and that they may be thus used for long periods without danger, which is not the case with the preparations of quinia. 6. That the sulphites can be administered without danger in spite of concomitant gastro-intestinal irritation and during the attack, and finally that many sequelæ of fever (excepting always anæmia) may be very advantageously treated with the sulphites. Since the sulphites have cured marsh fevers as well and perfectly as quinia, and have been found even still more serviceable than quinia in yellow fever, it has been suggested that the febrifuge action of quinia may be due to an anti-putrefactive action analogous to that which the sulphites exert on putrefying substances, and this view has been maintained and confirmed by M. Pavesi and M. Binz. It is not surprising now that carbolic acid, creasote, &c. have been used in similar cases with good effects. Arsenic perhaps acts in the same manner. The *curative* treatment adopted by M. Polli is given in the following prescriptions. If sulphite of soda be used, the proportion is 20 grammes of the salt in 200 of water, sweetened with honey or some aromatic syrup. This quantity is given in the course of twenty-four hours in divided doses. When the sulphite of magnesia is prescribed, he gives 12 grammes in the same quantity of water, taken in four or six doses; when the hyposulphite of soda, 15 grammes in 300 of water, taken in a similar manner. It is essential to take the remedy one hour before or two hours after a meal, and not to drink, except after a long interval, any acid substance, such as lemonade, or to take acid fruits or vinegar.

The *prophylactic* treatment.—For this purpose he prescribes 6 grammes of sulphite of magnesia, or 10 grammes of sulphite of soda, or 8 grammes of the hyposulphite of soda, in two doses, dissolved in water, morning and evening, and considers this sufficient to preserve an adult during the season favourable for endemic disease. This dose can be taken without inconvenience for several months together. M. Polli does not employ either the sulphite of potash or the sulphite of lime, the first having a disagreeable flavour and a too debilitating action; the latter being but little soluble and having also a disagreeable flavour. The hyposulphite of lime may, however, be administered, and is very useful in certain phases of tuberculous phthisis.—*The Practitioner*, Dec. 1871, from *Journal de Méd., du Chir., et de Pharmacol.*, Oct. 1871.

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35. *Phosphorus in Skin Diseases.*—Dr. EAMES communicated to the Medical Society of the College of Physicians, Ireland, a paper on this subject. It commenced with a brief review of the observations of Burgess, Broadbent, and Tilbury Fox on the employment of phosphorus as a substitute for arsenic in the treatment of many cases. Dr. Eames described his method of using the remedy. A solution of ten grains of phosphorus in one ounce of olive

oil was prepared, and of this a dose of from five to ten minims was administered three times a day; or capsules might be substituted in cases where the oily solution caused nausea or other unpleasant symptoms. Three sets of capsules, containing one-tenth, one-twentieth, and one-thirtieth of a grain of phosphorus respectively, had been made. The first case treated by Dr. Eames with the remedy was one of severe *acne indurata* of the face, of four years' standing. After six weeks, a cure was effected. In three cases of lupus, similar satisfactory results were obtained. In the first of these, a marked improvement was observed after a fortnight's trial, and the patient continued to take ten-minim doses of the phosphoretted oil for nine months. In the second instance, a five months' course of treatment was followed by cicatrization, and, eighteen months subsequently, there had been no return of the disease. In the third case, the oil was used during nine weeks, but with interruptions, owing to the appearance of grave dyspeptic symptoms. In one case of scrofuloderma, the glandular swellings disappeared in six weeks; in another, a cure was effected in three weeks. Psoriasis also yielded readily. In one instance of this affection, dyspepsia supervened on the administration of phosphorus, which was then temporarily stopped and the mineral acids given. A man, aged 24, with pemphigus, beginning on the abdomen, was quite well in a month. Cases of eczema of the scalp had also been much relieved. Dr. Eames referred to the silvery appearance of the tongue noticed when patients had been taking phosphorus for some time—another point of analogy with arsenic, and to the frequent occurrence of dyspepsia. The latter was to be met by the substitution of the mineral acids for a short time.—*Brit. Med. Journ.*, Dec. 9, 1871.

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36. *Treatment of Smallpox by Carbolic Acid.*—Dr. A. Löffler, of Stockenau, states, in the *Wiener Med. Wochenschrift* of February 10th, that he has treated more than forty cases of smallpox by the external copious application, by means of cotton-wool, of a solution of one part of carbolic acid in twelve of oil. The result in all the cases was, that the cutaneous swelling soon diminished; and that, when the application was made early, the course of the disease, in relation to the number of pustules, was milder. He believes also that by this treatment the danger of infection was greatly diminished. Unvaccinated children, inhabiting the same rooms with smallpox patients, either remained free from the disease, or had it in a very mild form. Carbolic acid was also diffused through the atmosphere of the sick-rooms.—*Brit. Med. Journ.*, February 17, 1872.

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37. *Warm Bath in Smallpox.*—In some notes on the treatment of smallpox in the *Dublin Journal of Medical Science* for January, Dr. Stokes, Regius Professor of Physic in the University of Dublin, lays great stress upon the use of the warm bath. He says: "We cannot doubt that the mortality in smallpox hospitals would be greatly diminished by the use of the bath." He describes a case in which the pustulation was almost universally confluent; the purulent matter highly putrescent; the hemorrhagic state developed; the body one universal ulcerous sore, and the blackness of the worst purpura developed; the odour of an intensely pungent and offensive character, which seemed to pass through the bystander like a sword. "Stimulants alone, freely and constantly employed, seemed to preserve the patient alive. The pulse was rapid, weak, and intermitting; and for several days we despaired of his life. At this juncture I happened to describe the case to my colleague, Mr. Smyly, who suggested the trial of the warm bath, with the view of relieving the terrible suffering. A bath in which he could recline was speedily procured; and, pillows being adjusted in it, we lifted the sufferer in, and placed him in the recumbent position. The effect was instantaneous and marvellous. The dilirium ceased as if by magic; it was the delirium of pain, and the patient exclaimed, 'Thank God! thank God! I am in Heaven! I am in Heaven! Why didn't you do this before?' The fetor immediately and completely disappeared, so that, on entering the ward, no one could suppose that there was a case of smallpox in it. He was kept at least seven hours in the bath, during which time brandy was freely administered, and omitted only when it showed symptoms of disagreeing with the brain. He was then removed to bed. The surface was

clean, and in many places the sores looked healthy and white. The bath was repeated next day, after which he fell, for the first time, into a tranquil slumber. From this time his recovery was progressive, delayed only by the formation of abscesses and the great soreness of the feet. That this gentleman's life would have been sacrificed but for the timely use of the bath, few who have had any experience in prognosis can reasonably doubt. He was in the condition of a patient every portion of whose skin had been burnt and ulcerated....This case and its singular result, in addition to the experience of Hebra, justifies the recommendation of the use of the bath. No danger attends its employment; and, in asthenic cases, stimulants can be freely used. In the Vienna Hospital, patients have been kept continuously in the bath for one hundred hours with good effect."—*Brit. Med. Journ.*, February 17, 1872.

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38. *Death from a Second Attack of Smallpox.*—REUSS relates, in the *General Report of Vaccinations performed during the year* 1869 (*Wurtemb. Med. Correspond. Blatt.*, 1871, No. 28, quoted in the 54th No., Dec. 30, 1871, of the *Centralblatt f. d. Med. Wissenschaften*), the case of a man 53 years of age, upon whose body were the unmistakable cicatrices resulting from a preceding attack of smallpox, who was attacked a second time with variola, terminating in death. In the accounts on record of cases where the individual experiences a second attack of smallpox, this latter, according to Dr. R., is always attended with increased danger. The correctness of which remark our own experience corroborates. D. F. C.

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39. *Revaccination.*—Dr. THEOPOLD (*Deutsche Klinik*, 1871, No. 38), a physician of Blomberg, of 30 years' standing, has had the opportunity of studying the circumstances connected with all the cases of variola and varioloid that have appeared in his vicinity during his term of professional service. He has, in very many instances, found the vesicles produced in cases of revaccination to be as full and perfect as in those following primary vaccination, and the lymph from them, when inserted in the arm, to furnish as complete protection from variolous infection. He suggests, therefore, that the lymph from a successfully revaccinated subject will furnish a valuable source of supply in case of the failure or scarcity of vaccine lymph from ordinary sources. In proof of the prophylactic efficiency of such lymph, the experience of the German army physicians is cited.

[There can be no doubt that in an extended series of revaccinations, in some cases, genuine vesicles, filled with effective lymph, may be met with; but the real question that presents itself, considering the great difference in point of infection which must exist necessarily in different cases of revaccination, is, will lymph thus obtained be safe to trust to for the protection of the system against the occurrence of smallpox? We should certainly answer, with Muller of Berlin, and with the mass of experienced vaccinators everywhere, in the negative.] D. F. C.

Dr. BEATTY stated at the meeting of the Medical Society of the College of Physicians (Jan. 17, 1872), that he had been very extensively revaccinating for the last couple of months. "Many of these were persons whom I had vaccinated in their youth, for I have lived long enough to see another generation spring up under my care. And I have been surprised at the way in which persons—generally boys and men whom I had vaccinated, and who bore most splendid marks, specimens that they would take to their graves of successful vaccination—have taken revaccination, and at the vesicles, perfect as those you would see on an infant, that have formed on their arms. In many cases of adults, aged from twenty to thirty, I have seen the most perfect vesicles formed; and in the cases of younger persons the vesicles have been so beautiful as to deserve to be drawn as specimens of the disease. I have revaccinated freely whenever I have been asked to do so, and I never saw a single instance of a bad result from revaccination. I have seen what are called sore arms, particularly on persons aged from twenty-five to thirty and thirty-five, where the vesicles did not form as well as in other cases; and a sort of spurious inflammation, or extension of inflammation round the wound, but nothing

of the slightest importance, or that a wet rag did not put an end to it in a couple of days. But I never saw a single bad result arising from revaccination. And the grand point is that of giving confidence to the individual, when you can at so little expense satisfy his mind and, moreover, satisfy your own mind; because if you refuse to vaccinate a person who asks you to do it, and that person subsequently gets smallpox, I need not tell you what you will get."

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40. *Glycerine-Lymph for Vaccination and Revaccination.*—Dr. Weiss had ample opportunity of testing the convenience and value, indeed, the indispensability almost, of the solution of vaccine lymph in glycerine, when a large number of persons were to be vaccinated or revaccinated with as little delay as possible, in the hospital under his charge, during a variolous epidemic which prevailed among the prisoners of war captured during the late Franco-German contest, and he bears unqualified testimony in its favour. In six cases where glycerin-lymph was ineffectually employed, the operation was subsequently repeated with unmixed, pure lymph from the vesicle, and equally without effect. The entire number revaccinated by Dr. W. was 5801. Of these, in 1586 cases, the operation was successful; thus, by revaccination were extinguished just that many foci which would otherwise have contributed to the further spread of the epidemic. Previously to the revaccinations being accomplished, the disease was continually on the increase, while subsequently, new cases quickly ceased to occur.—*Centralblatt f. d. Med. Wissenschaften*, 1871, No. 48, from *Eulenberg's Vierteljahr. f. Gericht. Med.* N. F. XV. D. F. C.

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41. *On the Use of Pepsine Wine in the Artificial Feeding of Infants.*—Dr. W. Jackson Cummins made an interesting communication on this subject to the Cork Pathological and Medico-Chirurgical Society. The value of pepsine, he remarked, in those forms of dyspepsia attended by a deficient secretion of gastric juice, is so well known and generally understood, that it is unnecessary for me to trespass on the time of the Society by more than an allusion to them. In the diseases of children, however, and especially as a substitute for a wet-nurse, when a mother is unable or unwilling to suckle her own child, the benefit of this valuable aid to digestion is not, I believe, as generally known, although allusions to it are to be found in medical essays. * * *

There is nothing of course like a good breast of milk for an infant, if it can be had; and in the "good old times," when the peasantry and small farmers lived on potatoes and milk, without stimulating their nerves with strong tea, nor their brains with penny-a-liner's novels, there was an ample field for the selection of a foster parent, but now even when that *rara avis*, a good nurse, is procured, she is so independent and knows her power so well, that any caprice must be humoured, and she is always ready to throw up her situation or neglect her charge.

A wet-nurse is, then, an admitted torment, and a balance struck between its advantage and disadvantage is generally against the former.

Artificial feeding by bottle is a great improvement upon the old system of spoon feeding, as the act of sucking stimulates the salivary glands and insures due insalivation, which is an important part of infantile digestion. With such an aid the stomach of most *human* infants is vigorous enough to fall into the way of digesting *cow's* milk, properly diluted, and mixed with sugar and cream to assimilate the proportion of its constituents to human milk—but besides the relative excess of casein and albumen contained in cow's milk when compared with human, the coagulum of the latter is "soft, flocculent, and not so thoroughly separated from the other elements of the fluid as the firm, hard curd of cows' milk is from the whey in which it floats."—(West.)

And when we reflect that the digestive organs of the *human* infant are found to digest human milk, and the force of its gastric juice proportioned to the solution of its soft flocculent coagulum, we can understand why the solvent power of its gastric juice is sometimes unequal to redigesting the firm curd of cow's milk. When such is the case, acetous fermentation is quickly set up, offensive gases distend the stomach and taint the breath, vomiting and diar-

rhœa set in, and in process of time the little patient sinks into a miserable state of marasmus, and dies.

The remedy for this state of things is simple, for although we cannot change the elementary composition of the milk we have to use, we can introduce into the infant's stomach a digestive power proportioned to the food it has to use —the organic principle of digestion taken from the stomach of the calf.

It is now many years since I first applied this simple theory to practice in the case of one of my own children, who, when about three or four months old, was reduced to a condition of marasmus by vomiting and diarrhœa, due to imperfect digestion of cow's milk. I ordered him fifteen or twenty drops of pepsine wine, to be given immediately before or after each meal. Soon after commencing it he began to improve, and by degrees all bad symptoms vanished, and nutrition was quite restored. The pepsine was continued until he was nearly two years old, and he throve at least as well as if he had been wet-nursed; other treatment of course preceded and accompanied the use of pepsine, but it was not until the latter was commenced that improvement took place.

Shortly after a child, born in England, and bottle-fed, was brought over to this country when about six months old; he also was suffering from infantile dyspepsia, and was pining away in a listless, apathetic state, quite indifferent to surrounding objects, and appearing as if he would lapse into idiocy from malnutrition of the nervous centres.

I immediately ordered him pepsine wine, which produced such beneficial effects that after it had been continued about twelve months, he had become a bright, intelligent, well-nourished child.

Since then I have never recommended a wet-nurse, and have used pepsine wine largely in dispensary, hospital, and private practice, and have seen many apparently hopeless cases recover under its use.—*Dub. Journ. Med. Sci.*, Feb. 1872.

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42. *Strychnia for the Relief of Obstinate Vomiting.*—M. DEBAUGE observes that, although nux vomica and strychnia have been employed in the treatment of various affections of the digestive organs, he is not aware of any account of strychnia being used for the relief of obstinate vomiting occurring in hysteria, pregnancy, suppressed menses, and disease of the uterus. This form of vomiting is dependent for the most part on asthenia, and occurs in debilitated subjects; and sometimes, after resisting all sedative remedies, it is arrested by the impression made on the gastric mucous membrane by stimulant drinks. In obstinate cases, however, these do not suffice, and then strychnia becomes a valuable remedy, and may be to this end administered endermically and hypodermically.—*Med. Times and Gaz.*, Feb. 17, 1872, from *Lyon Médicale*, Jan. 7, 1872.

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43. *Stimulating Hypodermic Injections.*—Some German journals have recently reported several interesting observations on the therapeutic value of stimulating hypodermic injections in different diseases of an asthenic type, and more especially in the typhoid fever, which prevailed in the Prussian army during the last campaign in France. Dr. ZUELZER has used with much advantage a new curative method in these maladies, viz., an hypodermic injection of six to eight drops of alcohol or liq. ammoniæ. By this method Dr. Zuelzer ascertained that the pulse from being small and irregular became quickly full and strong, that the cardiac contractions, at first weak and feeble, became regular, energetic, and visible to the eye, and, in fine, that the cyanosis and collapse quite disappeared. The small abscesses which sometimes form in consequence of the irritating nature of the injection are, it is stated, of no importance, as in the greater number of cases they are spontaneously resolved.—*Dub. Journ. Med. Sci.*, Feb. 1872, from *Lo Sperimentale*, July.

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44. *A Scarlet Efflorescence on the Skin produced by the External Application of Belladonna.*—Dr. J. G. WILSON records (*Glasgow Med. Journ.*, Feb. 1872) two cases of this. It has long been well known that belladonna administered internally will produce sometimes a scarlet rash on the skin, and we do

not remember to have heard of its external application being followed by such a result. Dr. W. says he has for several years past frequently and freely applied belladonna externally as an anti-lactescent, both in hospital and private practice. These two cases are the only instances in which he has observed any scarlatinoid rash result from its employment. In both these cases the eruption was extensive, and it was accompanied with redness of the fauces and dilated pupils.

SURGICAL PATHOLOGY AND THERAPEUTICS, AND OPERATIVE SURGERY.

45. *Science and Art of Healing of Wounds.*—Dr. B. W. Richardson read a paper on this subject before the Medical Society of London, January 29, 1872. He sketched the different methods at present adopted in the treatment of wounds, and the anomalies presented in the practice; dwelling first on the treatment of simple, and afterwards of complicated, wounds. In approaching the question of revision of this subject, the author held that it was necessary, first, to exclude all extrinsic or imaginary attempts at cure; and secondly, to avoid trusting blindly to what was called the *vis medicatrix naturæ*. Nature always pursued her own course, without respect to ease or pain, life or death. After this, Dr. Richardson illustrated from clinical observation the results of healing in extreme cases by the first intention; and inquired why, if in one case of the extreme kind there could be cure by the first intention, there should not be such cure in the majority of cases; why, in short, success by this method of cure should be the exception instead of the rule. Two sets of causes stood in the way: one remediable, the other irremediable. The remediable obstacles to recovery by the first intention were: want of care in bringing divided surfaces into perfect apposition; the too free use of water in dressing; the too prolonged exposure of wounds to the air; too much manipulation of the surface of the wound; the leaving of long ligatures within the wound; the imperfect closure of wounds from the air; the too hasty removal of dressings; and, most important of all, error of judgment on the part of the dresser in respect to the question whether there be, in the case to be treated, sufficient continuity of surface to warrant the attempt to heal by the first intention. The irremediable causes preventing healing by the first intention were: nervous lesions, influencing the vascular supply of the injured part; the accidental introduction of decomposing or other foreign matter; or the generation of organic poisonous products within the body. For the promotion of healing by the first intention, the author contended that the dressing employed should have four distinct qualities; it should be colloidal, elastic, impermeable to air, and styptic. He had himself introduced a fluid possessing these properties under the name of "styptic colloid," and it answered well; but he had no prejudice in respect to it specially, the principle on which it was constructed being conceded. Healing by the second intention formed the next part of Dr. Richardson's communication. Here again the rules named in relation to healing by the first intention were considered, and it was maintained that the same colloidal dressing was as effective in curing by the secondary as by the primary process, the difference of application being that the whole of the exposed surface was to be treated with the colloidal fluid. After giving a series of cases of rapid healing by the second intention, in some of which bone had been exposed and even joints laid open, and after touching briefly on the addition of iodine to colloidal solution in some particular cases, the author placed before the Society a summary of his arguments; and concluded by remarking that, if medical men could become unanimous in respect to the current progress of medical science and art; if in this simple matter of treating wounds, for instance, they could be of one mind, they would strengthen their bases of operation, clear up as they went on, and would go on more connectedly, noiselessly, and powerfully, towards new and certain conquest.—*British Medical Journal*, February 24, 1872.

46. *Sulphurous Acid Lotion in the Treatment of Contused Wounds.*—Dr. JOHN BALFOUR states that an extended experience has given him great faith in this application. It gives almost instant relief from pain, controls and greatly restrains suppurative action, and, where possible, secures primary union perhaps as efficiently as carbolic acid. The lotion is of the strength of one in twelve; a thin rag (the thinner the better) should be laid over the wound, and kept constantly wet for the first thirty-six to forty-eight hours. When cold becomes less agreeable, the lotion is used tepid, the rag being wetted every twelve hours and covered with gutta-percha. Where primary union is taking place, about the third or fourth day, a dressing of zinc ointment is to be substituted for the washing: this allows the skin to heal. When suppuration is established, a zinc lotion may be used after a week or ten days, and the cure wrought out on ordinary principles. Dr. Balfour records the following, amongst other cases: S. B., a lad between eleven and twelve years of age, on the 8th of June, in company with some other boys, was amusing himself with gunpowder; a "peeoge" (or devil) hung fire, and he poured some powder on it from the flask. This of course exploded, and tore open the metacarpal space between the thumb and forefinger of the right hand. The metacarpal bone of the thumb was fractured, and both wrists scorched. A mass of the short flexors of the thumb was forced out of the wound, contused, torn, and blackened. As this muscular substance was much injured and could not be returned without using undue force, a good deal of it was cut off; the wound was washed out with the sulphurous acid lotion, covered with a rag wet with the same, and the fracture was kept in position by tying the thumb to the forefinger. Had a fair night's rest; the wrists (not complained of yesterday) now painful and beginning to vesicate; dressed with carbolic acid and oil. Everything went on well, the burns on the wrists healed kindly, suppuration was most moderate, cicatrization rapid and perfect. Dr. Balfour lately passed the boy into a public work, with a thumb very little, if at all, the worse for the accident.—*Practitioner*, Dec. 1871, from *Edin. Med. and Surg. Journ.*, Nov. 1871.

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47. *Topical application of Cabbage Leaves to Wounds and Ulcers.*—Dr. BLANC extols (*Revue de Thérap.*, 15 Jan. 1872) the utility of cabbage leaves as a dressing for wounds, obstinate ulcers etc. The leaves should be deprived of their nervures, then applied with bandages, and the dressing repeated night and morning. He reports cases of wounds of various kinds, and obstinate ulcers, which were promptly cured by this application. He asserts that wounds of the most unfavourable appearance and giving off sanious discharges, promptly took on a healthy action and assumed a healthier aspect.

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48. *The Subcutaneous Injection of Morphia in Traumatic Erysipelas.*—Prof. ESTLANDER, of Helsingfors, states (*Deutsche Klinik*, No. 39) that he employed this injection originally in his clinical practice, in combination with the so-called abortive treatment (chiefly by means of tincture of iodine), mainly with the view of relieving the heat, tension, and pain of the inflamed skin. It was soon found, however, that the morphia must have exerted other effects also, so quickly was the course of the disease mitigated. It was therefore used in a series of cases as the sole local remedy, and the conviction became established that it must have exerted a direct influence on the inflammatory process, diminishing its intensity, and arresting its progress. When the limits between the inflamed and healthy portions of the skin are not very clearly defined, and the process manifests itself in the form of large red spots gradually approaching each other, if we inject near the affected parts, we usually find next day that the erysipelas has not extended farther, or has done so only to an insignificant extent. In cases in which the limits of the reddened and swollen skin are well marked, if we make some injections in its vicinity, we may find that the inflammatory process, which during the preceding twenty-four hours had made considerable progress, is sometimes at once arrested, but more frequently it continues in a diminished degree, gradually yielding in the course of a few days to a continuation of the treatment.

In the worst cases of erysipelas ambulans, as in the severe epidemic form, or

where a peculiar disposition of the individual prevails, the morphia exerts as little effect as any other of the so-termed abortive remedies. In estimating how far the results depend upon the peculiar nature of the erysipelas itself, and how much they are ascribable to the injections, Professor Estlander has undertaken many comparative trials, and he could relate many cases in which, while a rapid improvement followed the use of morphia, other cases treated at the same time, either expectantly or by means of other remedies, were much slower in their progress. Still, he is too well aware of the capricious character of erysipelas to venture to deliver any categorical judgment upon the subject. But a five-years experience has convinced him that these injections constitute a better mode of treating erysipelas than many other means.

For the injections, two grains of the chlorate or acetate of morphia are dissolved in a drachm of water; and as Luer's syringe holds about a quarter of a drachm, of which a quarter or a half is injected, it follows that the dose varies from one-eighth to one-quarter of a grain. As, so far from the erysipelas ever appearing at the small puncture-wounds, these and their immediate vicinity are always respected by it, the dose may be distributed over different parts of the healthy skin, at a distance of one or two inches from the limits of the inflammation. Usually the injection is made only once in the twenty-four hours.

Professor Estlander has no intention of proposing this as an exclusive method of treating erysipelas, believing, on the contrary, that one of its advantages is that it admits of the simultaneous use of other means. He has tried, indeed, all the various other remedies which have been recommended, and regards the tincture of iodine as the best of these. As soon as from shivering and the appearance of the wound erysipelas seems threatening, he administers an emetic, a means which he believes is now-a-days too much neglected, and one which he believes conduces to moderation of the disease. The morphia is next injected, either as the sole means or in conjunction with a daily painting with iodine, employing afterwards wadding and compression by a roller where practicable. Ipecacuanha with phosphoric or sulphuric acid may afterwards be administered. The sesquichloride of iron, once regarded as a specific, is of no real utility.—*Med. Times and Gaz.*, Dec. 9, 1871.

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49. *Abortive Treatment of Erysipelas by the Application of Silicate of Potash.*—Dr. Piazza extols the efficacy of silicate of potash in the cure of erysipelas. He applies two or three layers of the article on the affected parts, even when phlyctenæ are present. He treated by this plan an intense primary erysipelas of the face in a female, who was able to leave the hospital in four days, and during the whole treatment was able to nurse her infant. Dr. Figgioli has also rapidly and perfectly cured by this measure both traumatic and secondary erysipelas, and Dr. Piazza affirms that in his hospital it has become an established and successful mode of treatment, without having recourse to any internal general medication.—*Le Movement Médicale*, Dec. 3, 1871, from *Revue Méd. de Toulouse*, Sep. 1871.

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50. *Cancer treated by Condurango.*—Mr. J. W. Hulke read before the Clinical Society of London a paper on cases of cancer treated by condurango in the Middlesex Hospital. The author and his colleague, Mr. Campbell De Morgan, were enabled by the present of a parcel of condurango bark by an American surgeon to begin, in November last, a second trial of this reputed remedy for cancer. When this supply ran short, the trial was continued with bark bought of Messrs. John Bell & Co., and with a fluid extract very liberally placed at the author's disposal by its makers, Messrs. Bliss, Keene & Co., of New York. After briefly noticing its natural history, and the physiological and therapeutic properties assigned to the condurango, the author proceeded to relate two cases of ulcerated hard cancer of the female breast, and one of rodent cancer of the face, in all of which the exhibition of the reputed remedy failed to modify favourably, or to retard the progress of the disease. The result of this trial confirmed the author's first one, made in August and September last, which showed that, as a remedy for cancer, condurango was absolutely inert.

Mr. De Morgan also read a paper containing a report of three cases treated at the Middlesex Hospital, showing the uselessness of condurango: and he mentioned others which had come to his notice, tending to prove the same thing. He considered it very important that the fact should be widely made known that this and other so-called remedies for cancer had really no effect on the disease, as statements of wonderful cures were inducing the public to put faith in them, and to waste time and money in their trial. Of the cases on which he had tried the medicine, two were advanced cancers of the breast, and one was uterine. The patients were suffering generally from the effects of the disease, and were considered fit cases on which to try the experiment. The medicine was given regularly and carefully in the manner directed. In no one instance was there the slightest improvement in the conditions of the local disease, which advanced at the same rate as before; neither was there any diminution of pain or discharge, or any change for the better in the characters of the ulcerations. There was not, moreover, any general improvement. For a day or two they thought they had a better appetite, but this was the mere transient change one always sees in cancer patients. None of the changes which were said to take place in the conditions of the urine or the perspiration had been observed. Mr. De Morgan's impression was that the downward progress of these patients had not been arrested for one instant by the agency of the drug.—*Med. Times and Gazette*, Feb. 24, 1872.

51. *Thrombosis of Internal Carotid Artery.*—M. VERNEUIL communicated to the French Academy of Medicine, January 16, a highly interesting and rare case of this, the result of external injury, and which gave rise to anomalous symptoms and a faulty diagnosis. A man, aged forty-five, was brought, on December 14, to the Lariboisière, having been a short time before extricated from beneath a railway wagon which had been overturned. On his admission he was the subject of excessive agitation, which prevented any account of his injury being obtained from him. Gradually this disappeared, so that he was able to answer questions in the most clear manner. On examination, slight contusions on the vertex and elsewhere were found; but nothing could be discovered capable of explaining the signs of violent pain, the cries, disordered movements, and the great disturbance of respiration, of calorification, and of the circulation which he had exhibited on admission. These were regarded, therefore, as the effects of emotion; but in the evening he was seized with violent delirium, so as to require restraint, and this was followed in a few hours by profound coma and complete hemiplegia of the right side. His treatment, on account of his depressed condition, was merely expectant, and towards the fifth day he died. It was supposed that there had been laceration of a cerebral artery of small calibre, followed by slow effusion of blood; but at the autopsy not the slightest trace of effusion of blood or other lesion was at first discoverable. It was not until the base of the skull was examined that the true nature of the case was discovered. The internal carotid, on its entrance into the cranial cavity, was found to be filled with a thrombosis, which extended to all the ramifications of the middle cerebral artery, the carotid and Sylvian arteries seeming exactly as if they had been injected with suet. The anterior extremity of the left temporal lobe of the cerebrum was the seat of a widely extended ramollissement, fully explaining the hemiplegia observed. On following the track of the carotid downwards, it was found to be distended by a reddish, friable coagulum, until within a finger's breadth of the common carotid. From the base of the cranium to this point the artery was increased by at least one-third in its volume, suddenly diminishing in size there. On incising it at this spot, it was found that the internal tunics had been cleanly cut across, and, pushed back by the current of the blood, they had given rise to valvular folds, the free edge of which was turned towards the axis of the vessel. At this point the coagulated blood completely obliterated the calibre of the artery, the occlusion extending from below upwards to all the cervical and intra-cranial portions of the internal carotid, and also occupying the entire extent of the middle cerebral artery.

M. Verneuil suggests that it is probable that at the moment of the accident

a laceration of the carotid was caused by a forcible twisting movement of the neck, although the arterial tissue exhibited no morbid alteration favouring this. At first, blood might have been able to enter the encephalon; but, when occlusion was produced, the cerebral accidents supervened. The ramollissement came on on the fourth or fifth day, at the time when the thermometer indicated a notable rise of temperature. M. Verneuil pointed out to pathologists the importance of a minute examination of the cerebral arteries in analogous cases. In many of these an absence of lesions has been believed in solely from not having taken this precaution, while a more complete examination would have exhibited the material lesion.—*Med. Times and Gazette*, Feb. 3, 1872.

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52. *Resection of the Œsophagus.*—In the current number of Langenbeck's *Archiv*, Professor Billroth, of Vienna, contributes a most interesting and suggestive paper bearing the title, "*Ueber die Resection des Œsophagus.*" He states that some time ago, after a post-mortem examination of his first patient affected with carcinoma of the œsophagus, the possibility suggested itself of making a resection of this part of the alimentary tube. The fact that the lymphatic glands in the neighbourhood of the diseased part are not generally affected, and the partial success which had hitherto attended the operation of œsophagotomy in this disease, together with the analogy of external urethrotomy in cases of gangrene or ulceration of the urethra, seemed to lend support to such an idea. The passing, moreover, of bougies through cicatricial tissue was far preferable to the manipulation of such instruments in a tube with ulcerated and weakened walls.

On April 21st of last year, a large dog was put under the influence of chloroform, and a piece, about an inch and a half in length, was cut out of the whole circumference of the œsophagus. The lower end of the divided tube was then fastened by a couple of sutures to the skin at the margin of the external wound. Up to the 26th of the same month the animal was fed with milk through a tube passed into the wound, but on and after this date the tube was passed *via* the mouth. A week after the operation the sutures were removed. By the end of June the fistulous opening had completely closed, and the process of healing would have been quicker if it had not been that the dog, like human patients, dissatisfied with "milk diet," purloined the more solid food of neighbouring victims to science. After the closure of the œsophageal fistula, which took place at the end of June, the tube was daily dilated by a bougie of the diameter of a large index finger. After the healing of the wound the dog was in capital condition, eating meat, potatoes, etc., but the variety of fare was not allowed to extend to bones. On July 26th the animal was killed with cyanide of potassium; and all that was found as a trace of the operation was an annular scar, scarcely half a line in width, and, moreover, easily dilatable.—*Lancet*, Jan. 6, 1872.

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53. *Gunshot Wounds of the Brain.*—Prof. Podrazki narrates (*Weiner Medizin. Wochenschrift*, Dec. 9 and 16, 1871) an interesting case of this.

Early in October, 1869, a youth, aged 15, was accidentally shot in the head by a pistol, and on Professor Podrazki seeing him next day he found a round wound one centimetre in diameter, situate at the internal end of the left arcus superciliaris, into which he could pass the end of his little finger. At its bottom he found some coagula, and could feel a slight pulsating movement. The bony edges of the wound were sharp and jagged, and the surrounding skin was blackened by the powder. The lad, who was robust and healthy, lay pale and senseless, the pupils being widely dilated and in nowise sensitive to light. The breathing was stertorous, and the pulse, which was very compressible, beat only 54. A small quantity of slightly blood-coloured serum flowed from the wound. Both extremities were completely paralyzed on the right side, and the urine had passed away involuntarily. The practitioner who had first seen him had passed a probe some five or six inches long into the track of the wound (which ran horizontally backwards) without coming in contact with the ball. The diagnosis here was easy, all the symptoms indicating that the projectile had probably done excessive injury to the left anterior lobe of the brain, and that

this had been attended with a considerable extravasation of blood either into the substance of the brain or between its surface and the bony coverings. Where the ball might be could not be conjectured. Of course, the prognosis was highly unfavourable. The treatment was expectant, ice being applied to the head, strong soup administered, and cold-water clysters given as an occasional aperient. Searching for the ball was out of the question, and the locality of the wound prevented the patient being placed in a position favouring the action of gravity.

In a few days the patient's consciousness gradually returned, so that he became able to answer questions, but only after long consideration, as if he were in search for words, and then very slowly—in complete contrast with his ordinary quick and lively speech. In about a week some pain in the head appeared, accompanied by a pulse of 130, regular morning vomiting, and sleepless nights. There was occasional delirium, and the palsied limbs were very tremulous. The discharge from the wound consisted in part of genuine pus, and in part of serous fluid. This condition of things had several times alternated with a remarkable remission of disquieting symptoms, when suddenly, about the middle of November, a soft, pale-red tumour made its appearance, projecting from the wound, but gradually became covered over by the granulations and skin, which formed over it a tolerably firm cicatrix. At first only of the size of a hazel-nut, it gradually but slowly increased. Simultaneously with its appearance the condition of the patient exhibited a most marked improvement, his pulse going down to 80, his consciousness being clear, his sleep and appetite excellent. Even the paralysis exhibited decided improvement.

On December 12—two months after the accident—Professor Podrazki was alarmed at learning that the surgeon in attendance had mistaken this tumour lying over the healed wound for an encysted tumour, and had proceeded to remove it. Alarmed, however, by the large quantity of black blood that issued on an incision of the skin, he desisted from the attempt. The author, on arriving, found a hernia cerebri of the size of a small walnut, which had become covered by a firm cicatrix. This having been cut through, as stated above, the pure cerebral mass projected. An attempt at returning this having been attended by the sinking of the pulse from 90 to 54, and by the patient becoming faint and cyanotic, it was determined to leave it as it was, only covering it with oiled linen and wadding. At the same time a remarkable appearance was discovered a little above the external occipital protuberance—a small, elastic tumour, the size of a bean, which, on pressure, yielded somewhat and felt painful. Spontaneous pains had, it seems, all along been occasionally felt here. The bones appeared entirely intact; but as this small tumour corresponded pretty nearly to the spot at which the ball coursing along the inner surface of the bones might be expected to reach, the question arose whether the swelling was not connected with the presence of the ball, and whether trephining would not be justifiable in order to secure its removal and to give issue to the pus, which possibly was the cause of the hernia cerebri. This procedure Professor Podrazki considered too conjectural, and at all events not to be had recourse to unless symptoms of cerebral compression came on. The patient at that time was improving every day, the paralysis having much diminished, so that he was able to sit up for some hours daily. He had, however, entirely lost the power of memory for recent occurrences and for things that had happened only a few hours, while remote events were well recollected. His visual power was also to some extent enfeebled. It was resolved not to interfere. There was a continuous flow of a clear, serous fluid (cerebro-spinal?) from the wound made by the incision, and the hernia cerebri—or, as Professor Podrazki prefers calling it, the prolapsus of the brain—continued to increase, until it had attained the size of a walnut, projecting over the incision-wound. By July, 1870, complete cicatrization had taken place, and a photograph is given of the lad's appearance at that time. The tumour measured two inches in length, and was one inch and a half in thickness, being quite transparent, and very elastic in its anterior part. At its base the sharp edge of the bony aperture could be felt, surrounded at its lower portion by somewhat pointed osteophytes. Compression employed for some minutes diminished the size of the tumour remark-

ably, this becoming soft, and having a faint pulsation, while the usually excessively distended skin was thrown into folds. This pressure, however, threw the patient into a state of syncope, his pulse becoming remarkably slow. The tumour evidently only in part consisted of cerebral substance, the remainder (in its anterior portion) being cerebro-spinal fluid; and the author resolved, in consultation with Hofrath von Pitha, to resort to puncture. This he executed in November, 1870, by means of Dieulafoy's *aspirateur pneumatique*, and the tumour immediately diminished to half its size, while the remainder was, by means of gradual and gentle pressure, entirely returned, without any symptoms of cerebral pressure being produced. A pad was applied to retain it. Quite recently the author has heard from the lad's friends that nothing now remains of the tumour, the forehead being quite even, the aperture in the bone being filled up with a firm cartilage-like substance. The general health is excellent, and the patient is able to walk about, though only slowly, as the power of the right foot is not yet quite restored, although the paralysis of the arm has completely disappeared. Speech is still slow and difficult, and has become remarkably deep. The intellectual power has not yet diminished.

Professor Podrazki observes that the friends of the lad regard him as cured, but for the surgeon he is still a subject of anxiety; and it is only in a qualified manner that he reports the case as a "recovery." Indeed, in some interesting general observations with which he prefaces the narrative of this case, it is evident that the greatest caution must be observed in delivering our prognosis in this class of injuries. We have only space to briefly allude to some of the remarks in question. Sometimes, it is observed, death takes place only weeks or months after gunshot wounds of the head, either in consequence of secondary inflammation having been set up, an abscess having formed in the track of the wound, or from decomposition of extravasated blood. As long as the ball remains within the skull, life is permanently menaced, although the wound may have completely healed, and all cerebral disturbance have disappeared. Various authors have recorded cases in which, even years after apparently complete recovery, death has taken place, either suddenly without preliminary symptoms, or under an apoplectic attack, or with a gradual loss of strength. In the great majority of cases, acute encephalitis or meningitis occurs, the patient dying with symptoms of compression of the brain from the formation of matter, or with those of pyæmia. Sometimes death takes place quite suddenly, when all seems to be going on quite well. This was so in a soldier from the Italian war, who, standing upright by his bed, and questioned as to his ailments, laconically replied that he had a ball in his head. And so it proved. The pulsations of the brain were visible through an aperture in the frontal bone. His pulse beat only 44, but otherwise he seemed very well, and was making decided progress, when, on standing up, he fell down suddenly, dead. An abscess was found in the anterior lobe of the brain, and the ball lay at the bottom of it. In other cases the ball sinks by its own weight through the soft mass of the brain until coming in contact with some important part, it at once causes death. Sometimes it reaches the base of the skull, where it remains long without doing mischief, or may become eliminated—although this is one of the rarest occurrences. Patients are placed in the greatest danger by attempts being made to search for the position of a ball by means of a probe; for this may easily, without being perceived, penetrate uninjured portions of the soft mass of the brain, and give rise to immediate death. Whenever the probe is used, all pressure must be abstained from, the instrument being left to glide along. The occurrence of prolapsus or hernia cerebri, which usually takes place only some weeks after the occurrence of the injury, adds much to the gravity of the prognosis. According to Bruns, about two-thirds of those who exhibit this die, and Professor Podrazki's experience leads him to consider this a too favourable statement. The seven cases which have occurred in his clinic at the Joseph's Academy between 1854 and 1870 have all died.—*Med. Times and Gaz.*, Feb. 17, 1870.

54. *Excision of the Diaphysis of the Humerus after Fractures from Gunshot.*—M. Ollier read recently before the Lyons Society of Medicine a very

interesting memoir on this subject. He had performed the operation in three cases during the late campaign on the Loire, removing nine, seven, and six centimetres of the bone; and all the patients did well. In one of these the excision of six centimetres was performed on the upper third of the humerus, below the head, which firmly united with the rest of the bone, and the uses of the limb were completely re-established, the shortening which remained being only twenty-five millimetres.

The cases of gunshot wound of the humerus, M. Ollier observed, in which his operation is indicated are comparatively rare, and expectation should be the general rule. It is in comminuted fracture, with denudation of the periosteum and contusion of the medulla, and especially when the projectile remains amidst or in the vicinity of the fragments, that intervention should take place. At a later period inflammation and pain may also render excision necessary; for one of the immediate consequences of the operation is the disappearance of the pains, to the great relief of the patient. M. Ollier's mode of procedure is entirely different from the old one for the removal of fragments of bone; for, in spite of the comminution, a true sub-periosteal excision should be attempted. Each fragment is successively seized and separated from its periosteum, so that at last a tolerably complete periosteal sheath is obtained, in spite of its lacerations opposite the seat of fracture. The bone, in fact, being a very compact body, is broken into a number of fragments completely separated from each other, while the more supple periosteum resists. It becomes more or less torn, and remains adherent to the soft parts and the fragments, especially in young subjects. The modifications produced in the periosteal adhesions by age are indeed considerable, and are very important as regards operations. When the splinters have been removed, the fragments must be excised to beyond the extent of the fissures. If, however, the fissure extends to the spongy tissue, and the subject is young, it need not be pursued if the soft parts are intact; but when it penetrates to a joint, an articular excision must be executed. After an excision, the ends of the bone should be brought nearer to each other, in proportion as there is little expectation of bony reproduction, which is less in proportion to the age of the patient. The silicated bandage favours the reparative process, and may require to be continued for months. In answer to a question as to the prevention of stiffness of the joints ensuing, M. Ollier replied, that, so long as the inflammation persists, the bandage must be left on, it being indeed the best means of limiting the traumatic inflammation; and whenever it is renewed, movement should be imparted to the elbow and shoulder. The apparatus may, in fact, be left on without renewal for a month or five weeks, and anchylosis will usually be avoided, unless the fracture is too close to the joint. In conclusion, he repeated that expectation is the rule, and that excision is only suited for particular cases. It is especially indicated when, some time after the accident, complications arise; while, when there is intra-articular fracture, it should be performed immediately. The influence of age must also never be forgotten.—*Med. Times and Gazette*, Feb. 24, 1872.

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55. *Hydrarthrosis of the Knee in Fracture of the Femur.*—On the occasion of a communication presented by M. Gayet to the Lyons Society of Medicine, "On Hydrarthrosis of the Knee in Fractures of the Femur," M. Ollier observed that he had long observed this circumstance, but did not consider it as peculiar to the knee-joint. Indeed, in the articles "Ankyloses" and "Arthrite," in the *Dictionnaire des Sciences Médicales*, now publishing, he has shown that such swelling may effect various joints, the amount of effusion being proportionate to that of the irritation produced by the fracture. Ordinarily, it is serous in its nature, but it may become purulent, notwithstanding its distance from the seat of fracture, when this is the seat of violent inflammation. M. Ollier refers these effusions to the propagation of irritation through the osseous tissue, and has described them under the name of *arthrites par propagation*, or *secondaires*. What led him to take this view of their nature was that he had observed them to be produced experimentally in other traumatic lesions of the bones, which could only operate by transmitted irritation, as after breaking up and evacuating the medulla through perforations made in

the bone. He is unable, therefore, to agree with M. Gayet's view, that these effusions are a consequence of obstacles to the return-circulation in the vessels of the medulla. The irritation transmitted through the vascular tissues of the bone gives rise to an increase of the normal secretion of the synovial membrane, and the same thing is observed in some forms of coxalgia. M. Ollier does not consider that the effusion in the knee is here connected with the synovitis of the hip-joint, but with the juxta-epiphysary osteitis of the upper part of the femur, which, in consequence of the relation of the diaphysis with the articulation, soon becomes confounded with the true coxalgia. The propagation of the irritation through the femur seems also, to him, to furnish the most general explanation of the persistent pains in the knee observed in coxalgia. With M. Gayet, M. Ollier believes that, in some exceptional cases, hydrarthrosis of the knee may assist in completing the diagnosis of fracture of the femur. But this is only an example of what is met with, more or less, in all joints which form the limits of a fractured bone, being more easily recognizable in the knee in consequence of the extent and superficial position of its synovial membrane. M. Ollier has observed it distinctly, also in the elbow and wrist after fracture of the ulna, and in the instep after fracture of the shaft of the tibia. If femero-tibial hydrarthrosis is less frequent after fractures of the tibia than after those of the femur, this is because the vessels of the synovial membrane and of the articular soft parts have more numerous anastomoses with those of the femur than with those of the tibia. M. Ollier regards the proposition of removing the effused fluid by means of an aspirator as useless, inasmuch as it is generally absorbed in the course of a few days. In removing it we should transfer a serous into a dry arthritis—that is, we should facilitate the production of anchylosis, or at all events the stiffness of the joint, which will take place as long as the synovial membrane is distended with fluid.

M. Delore observed that he had often met with this hydrarthrosis of the knee in fracture of the femur, but he does not believe that it should be employed as a sign of such fracture, as it may be produced by a simultaneous contusion or sprain. He cannot admit the theory of transmitted osseous irritation, as it may supervene very rapidly after the fracture. M. Gayet explains the hydrarthrosis by an obstacle to the medullary circulation only; but it would seem more reasonable to admit an impediment in the circulation in the whole substance of the limb, induced by an extra-osseous effusion of blood which takes place in all fractures. M. Delore also refers to the phlyctenæ which are sometimes produced, containing either blood or more or less coloured serosity, and which, he believes, are due to the same cause as hydrarthrosis. He is certain that the hydrarthrosis is very seldom dependent upon arthritis, and that very rarely can the pain in the knee, observed in coxalgia, be attributed to arthritis. Very often we are able to squeeze the knee without giving rise to any pain, while this immediately appears if we apply pressure at the hip. Moreover, these pains are found not only in the knee but also in the leg, or even in the foot. In his opinion they are sympathetic. M. Ollier added, in explanation, that these effusions are to be distinguished according to the period when they occur. Those which are produced immediately, or rapidly, are due to distension or spraining of the knee, which accompanies most fractures; while those which come on more slowly are the products of propagated irritation. The former result from laceration of the capsule and synovial membrane, more or less blood being always added to the effusion; the latter, which are simply the result of propagated irritation, consist of a more or less transparent serosity, as in any other case of serous arthritis.—*Med. Times and Gaz.*, Dec. 30, 1871.

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56. *Ovariotomy during Pregnancy.*—Dr. Eugene Goddard, at a late meeting of the Obstetrical Society of London (*Med. Times and Gaz.*, Jan. 6, 1872), narrated the following case of this: The patient was 29 years of age, and in 1870 was found to be the subject of an ovarian cyst, but as there were no urgent symptoms, the consideration of any surgical treatment was deferred. She then became pregnant, and about the end of the second month of utero-gestation, Mr. Spencer Wells removed the ovarian cyst. Eleven and a half

pints of fluid were withdrawn. The clamp was removed, and the bowels acted on the eighth day. Pregnancy went on uninterruptedly, and a living child was born at the full period. Dr. Goddard said that the compound nature of the cyst precluded the idea of tapping, as also did the risk of peritonitis, suppuration of the cyst, and the formation of adhesions. Premature labour was not induced, because the patient was already beginning to suffer constitutional disturbance from the double burden, and it was doubtful whether, by the time a viable child could be born, they would have assumed such magnitude as to imperil the patient's safety; whereas, if abortion were induced, the child would be lost and the tumour would remain.

Dr. Ross related a case in which Mr. Wells had operated under more adverse circumstances, as the lady was much broken down in health at the time of the operation. A small ovarian tumour was diagnosed eighteen years ago. The patient subsequently got married, and Dr. Ross had attended her in four labours. In no instance was parturition attended with any serious difficulty. It was observed that during gestation the tumour appeared to become smaller. The tumour rapidly increased about a year ago, and Mr. Wells removed it successfully, the patient being about two months pregnant. Her labour is now daily expected.

Mr. Spencer Wells said that the existence of the cyst for eighteen years, and the presence in its walls of hard, bone-like masses, had led to the diagnosis of a dermoid tumour. Mr. Wells had performed ovariotomy four times during pregnancy, and all the patients had recovered.

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57. *Traumatic Tetanus.*—Dr. J. FAYRER records (*Indian Med. Gaz.*, Feb. 1872) three cases of traumatic tetanus; one resulting from lacerated wound of right great toe, the second from a wound of the left index finger, and the third from wound on dorsum of right foot. Cases one and two recovered from well-marked symptoms of the disease after amputation of the wounded part in which the disease originated. In the third case, removal of the cicatrix was not successful, the disease progressed, and the patient died.

In two cases there could be no doubt, Dr. F. says, that the origin of the disease was completely removed, for as the wound was situated on the digitis, and those parts were amputated, the injured afferent nerves, viz., those which conveyed the evil influence to the nerve centres, were necessarily divided. The source of disturbance being removed, the symptoms subsided and the patients recovered. I have, on a former occasion, recorded a similar case in which a favourable issue resulted, and those now noted tend to prove that if the part whence the irritation sets out, and which, if allowed to remain, excites that peculiar polar condition of the cord which results in the waste of nerve force, tetanus, be removed early, the perturbation, though great, is not necessarily permanent, and may subside.

Though no one would hesitate to amputate a finger or toe in such a case, most would hesitate to apply the same treatment in the case of a limb, until the symptoms of tetanic spasm became so severe as to leave no doubt as to the greater danger incurred from the disease: unfortunately it is then too late; the disease is thoroughly established, and amputation is as powerless to remove the results as any other remedy.

In so desperate a disease as traumatic tetanus, the most desperate remedies are justifiable, and I would suggest the advantage of resorting to amputation, even of a leg, on the earliest invasion of the symptoms.

Amputation, I am quite aware, has often been practised and with unfavourable results, but I am inclined to believe that it has not, as a general rule, been resorted to early enough to give it a fair chance of success.

No one would recommend amputation of the limb if the peccant nerve could be isolated and divided, as in the case of that leading from a finger or a toe.

In a wound of the leg or arm it is almost impossible to indicate the branch or trunk that is the conductor of mischief to the center; and therefore to secure its division the whole limb must be removed.

The hydrate of chloral may have had something to say to the recovery in these cases, but I have not found it successful except as euthanasia, in these cases.

58. *New Treatment of Traumatic Tetanus.*—M. Demarquay recently addressed a short communication to the Académie des Sciences, giving an account of a new mode which he has adopted of treating traumatic tetanus. Having, he says, during the late siege, lost many cases without being able to alleviate them, he resolved in future to try a new procedure. First bearing in mind the great susceptibility to cold manifested by these patients, and the aggravation of the suffering which this produced, he kept the two cases he now reports upon in a room heated to and carefully kept at a temperature of from 18° to 22° C. (64° to 72° F.). Next, in order to diminish the tonic and clonic contractions, which are in this disease so painful, causing the patient to assume such strange positions, and especially to subdue the trismus, which is one of the earliest manifestations of tetanus, as well as to relieve the pain of the wound and the convulsions of the stump, he performed, four or five times in the twenty-four hours, intra-muscular injections, as near as possible to the emergence of the nerves. These consisted of solutions of morphia diluted to a fiftieth part. At first each masseter was injected, as also the muscles of the neck on each side of the spinal column; and when the wound which had been the occasional cause of the tetanus was painful, an injection was thrown deeply into the substance of the muscles in its vicinity. Under the influence of these remedies the sufferings were speedily assuaged, and the patient was enabled to open the mouth, and by copious drinks relieve the tormenting thirst. By aid of these, too, and the raised temperature of the room, abundant transpiration was produced. After some hours the injections were repeated, the painful contractions being pursued wherever they appeared, throwing them into the substance of the muscles concerned. They were also made over the track of the nerves of the diaphragm, to subdue the spasm of this muscle, or along the course of the pneumogastric, with the view of relieving the difficulty of deglutition, which appears to depend upon contraction of the œsophagus. In this way the pains were assuaged and the thirst relieved, while the patient was able to be fed with broths, milk, and an increasingly substantial diet. One of the two cases was suffering when seized with tetanus from a deep wound of the calf in process of cure, while the other had undergone amputation of the leg. In both, the tetanus, to all appearance, was very severe; and although, of course, two cases do not say much in favour of any mode of treatment, their successful issue justifies its being made known. Frequent subcutaneous injections of morphia, atropia, and curare have been tried, but, as far as M. Demarquay is aware, no one has hitherto thought of carrying the curative agent deeply into the substance of the muscles. This is, he believes, to be both a novel and rational procedure.—*Med. Times and Gaz.*, Oct. 7, 1871.

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59. *Transfusion.*—The *Dublin Journal of Medical Science* (January, 1872) contains two papers on this subject read before the Dublin Obstetrical Society, by Dr. A. H. Ringland, the other by John Ringland, M.D., with a report of the discussion to which the reading of these papers gave rise. Dr. A. H. Ringland's paper presents a very interesting history of the operation. Dr. John Ringland relates a case in which transfusion was successfully resorted to in a case of post-partum hemorrhage.

Dr. Robert McDonnell made some interesting remarks on the method of performing transfusion. He said "there were some operations, such as those for the removal of tumours and for aneurisms, &c., which could not be practised on the dead subject. There were others, of which transfusion was one, which could be thus practised; and that being so, it was inexcusable for any young surgeon not to avail himself of his opportunities to make himself familiar with the details of an operation which he might afterwards find necessary for the preservation of life. This was essentially an operation of detail. In the first instance perfect cleanliness was indispensable. All the instruments, the tubing, the pipette, should be perfectly clean; the bowl in which the blood was received should be scalded with hot water, and the muslin used in straining the blood should be scalded also. In the second place, the only really difficult part of the operation when it came to be performed was the opening of the vein in the person into whom the blood was to be transfused. It was like taking up a vein

in the dead body. There was this only difference between the dead and the living bodies—that in the latter, owing to the cold air getting at the vein, its muscular fibres contract, and it becomes very small. Hence the importance of passing a needle under it, so that one might always be able to find it when necessary. He believed that in both the cases that were detailed that evening the apparent difficulty found in putting in the blood arose from their natural anxiety to see it flow in quickly. You think the blood is not going in, when in reality it is going in, but very slowly. He had never found any instance, when practising on the dead subject, in which the weight of the blood was not of itself sufficient to make the blood run in without difficulty. The nozzle which had been described by him in the *Dublin Quarterly Journal* was an important part of the instrument.

It is of great importance to have a canula, with a probe point, and the eyelet hole at the side, not at the end. The probe point was best suited for entering a small opening. They could easily make sure that all air was expelled before they put it in, and if, during the progress of the operation, a bubble of air was observed, it was possible to withdraw it, and allow the air to escape without any serious interruption of the operation. There was another advantage in an apparatus of the kind exhibited, well illustrated in the case detailed by Dr. Ringland. There was some delay experienced in getting at the vein in the woman's arm when all was ready for making the injection. In such a case it was only necessary to place the nozzle in the top of the tube, and the pipette could then be placed in a can of hot water, and allowed to remain there as long as the operator wished. It was evident that this was a great advantage, inasmuch as everything could be got ready for use without undue haste, and the operation could be carefully and deliberately performed without fear of the blood getting cold. He believed this operation was capable of being applied to a large number of cases besides those with which the members of that Society were so familiar in obstetric practice. He hoped it might be applied both in surgical and medical cases, not only in cases where accidents or ulcers had caused copious hemorrhage, but in cases of the nature of chlorosis or of cholera. Where an injection of saline solutions into the veins might be considered advisable, the appliances, for the purpose of transfusion being now so much simplified, should encourage surgeons to lend their aid to physicians in cases of that kind. He believed the most important thing connected with this operation was the establishing the desirability of introducing *defibrinated* blood. "The risk of trying to inject blood not defibrinated was very great, and he was of opinion that the deaths which occurred several days after transfusion, occurred from embolism. He said then that, physiologically, the defibrination of the blood made it better for the patient, and surgically it disarmed the operation of most of its dangers and risks. With regard to the difficulty experienced in Dr. Ringland's case in getting the blood in, he was inclined to think that in all these cases it arose from their being rather precipitate, and thinking that the blood should go in faster than it really should. He had contrived a simple apparatus, by which a good deal more force could be put on, as in Bellina's apparatus by an India-rubber bottle; but at the same time he did not recommend it. It was important that the blood should not flow in too quickly, and he believed that gravitation alone was sufficient to get it in—at least gravitation aided by the pressure of the mouth. Care should be taken not to have the blood too hot. The operator should be provided with a thermometer in every case, otherwise he ran the risk of producing coagulation of the albumen. The blood should be heated from 100° to 105°, at which there was no risk of coagulation, and this temperature could be easily maintained.

OPHTHALMOLOGY.

60. *Neurosis of the Optic Nerve and Retina.*—Dr. Herman Pagenstecher narrates the case of a girl twelve years of age, who was brought to him for advice on account of rapid diminution of vision of four weeks' standing. For a year previously she had suffered from nervous symptoms—attacks of lameness, first of one leg, then the other, headaches, vomiting, etc., which always yielded to strychnia. On examination, vision was found very defective in both eyes: they were sensitive to bright light, and moderate darkening of the room improved the vision slightly; but the application of either a red or blue glass at once brought the vision almost to a normal standard. There was hyperæsthesia of the whole of the left side of the body, and pinching or striking any part of the left side induced reflex movements and appearance of bright spectra before the left eye. Blue-coloured glasses were ordered. Three days later the acuity of vision was found to be normal with blue glasses. The hyperæsthesia existed as before. The coloured spectra that appeared before the eye on pinching the left side of the head, were described by the patient as consisting of concentric rings—the outermost red, the innermost yellow, the intermediate black. Pressure over the back of the head and first three cervical vertebræ induced severe pain, and produced the brightest-coloured spectra. Pressure below the ninth dorsal vertebra on the left side produced merely the appearance of a red disk before the eye. During the next ten days the hyperæsthesia gradually diminished in degree, but was still markedly present at the end of that time. Dr. W. Müller saw the case, and applied the constant galvanic current, with the following effect: On applying the copper pole to the back of the head and the spinous processes of the upper cervical vertebræ, while the zinc pole was held in her hand, the weakest current produced the coloured rings above described during the whole period of the passage of the current. Applied below the seventh cervical vertebra, only the red colour was produced; below the third dorsal vertebra, no spectrum was produced. On direct stimulation of the right optic nerve by applying either the copper or zinc pole to the immediate neighbourhood of the eye, a yellow glimmer appeared on opening and closing the current, which, however, was persistent when the current was applied to the left optic nerve; the same occurred on passing the current through the head. Irritation of the sympathetic by applying one pole to the angle of the jaw, the other over the first cervical vertebra, induced a yellow colour. After stimulating the medulla for half a minute, all appearance of coloured spectra vanished, and they could not be reproduced by passing the current in any direction. During the course of the examination, the patient stated that the colours became less and less distinct, till yellow took the place of the other colours, and eventually it disappeared. The following day, pressure over the back of the head reproduced the colours as before. The constant stream, with the application of the copper pole over the medulla, was daily employed, and in the course of a few days the affection entirely disappeared.—*Edin. Med. Journ.*, Nov. 1871, from *Zehender's Monatsblätt für Augenheilkunde*, Jan. 1871.

61. *Syphilitic Amblyopia and Amaurosis.*—M. Galezowski has published in the *Archives Gén. de Médecine* (first three months of 1871) an essay on this subject, the deductions of which are as follows: 1. Syphilitic retinitis and neuritis may exist without any alteration of the choroid, and are mostly constituted by an apoplectic retinitis with exudation. 2. Syphilitic retinitis presents no pathognomonic symptoms by which it may be distinguished from other kinds of retinitis. 3. But when retinitis or optic neuritis is accompanied by iritis or choroiditis, with or without flakes in the vitreous, the affection is, without doubt, syphilitic. He has always found that no other affection, save glaucoma, can give rise simultaneously to apoplexy of the retina and to iritis or choroiditis. 4. The derangement of the chromatic faculty is ever present in these two forms of ocular alterations, and especially in optic neuritis. 5. These complaints are best treated by large doses of iodide of potassium and corrosive

sublimate (one drachm and a half of iodide of potassium, and from three-quarters of a grain to one grain of the sublimate per diem). 6. Syphilitic choroiditis is the most frequent form of syphilitic amblyopia and amaurosis. The signs of this choroiditis are very characteristic, and, as it were, pathognomonic of syphilis. They are as follows: *a*. Disturbance or loss of vision, occurring in fits, often at long intervals; *b*, a cloud in the shape of a cobweb constantly floating before the eyes; *c*, very frequent photoptics; *d*, photophobia; *e*, hemeralopia at an advanced stage of the complaint; *f*, preservation for a long time of central vision, with diminution of peripheric field; *g*, cloudy papilla; *h*, pigmentary retinitis at a more advanced period of the complaint; *i*, atrophy of the central vessels of the papilla with preservation of the rosy tint depending on the trophic or cerebral vessels of the optic nerve. 7. Pigmentary retinitis occurs very often as a sequel to syphilitic choroiditis. 8. The syphilitic pigmentary stains occur along the vessels of the retina, but they form, besides, circular masses in the shape of herpes circinatus. 9. Pigmentary syphilitic retinitis, the result of contamination, differs from congenital pigmentary retinitis (especially that form hitherto attributed to the consanguinity of the parents) only by the circular form of the pigmentary stains. 10. Congenital pigmentary retinitis is a hereditary syphilitic affection. 11. The latter complaint should, at a tender age, be treated by mercury or iodine. After a certain age the progress of the complaint cannot be stopped; it goes on increasing, and is followed at a more or less advanced period by loss of vision. 12. Children born of syphilitic parents should, at birth, be examined with the ophthalmoscope. If retinitis be found, the means just mentioned should be used.—*The Lancet*, Dec. 9, 1871.

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62. *Amblyopia from White Atrophy of Optic Nerve greatly benefited by the Continuous Galvanic Current.*—An extremely interesting case of this is recorded by Dr. DONALD FRASER, of Paisley, in the *Glasgow Medical Journal* for February last. The subject of it was a weaver, æt. 59; had always been a healthy man, and temperate; for the last ten years has smoked two and a half ounces of tobacco per week. At the age of 44 he began to use spectacles for presbyopia. For the last nine years of his work as a weaver, he taxed his eyes severely at pattern weaving, working most of the day, during the greater part of the year, in gaslight. For the last five years his sight had been gradually failing. It was not, however, until the beginning of last year that he began to be alarmed at the rapid increase of this failure. He now became dyspeptic, low spirited, and weak; all this due, he considered, to the depressing effects of some family troubles. At this time, during the day, a mist came before his eyes, which passed away at twilight, so that by gaslight he could read, for a few minutes at a time, the largest type of the newspapers with No. 6 convex glasses. About nine months ago even this became impossible. When he consulted Dr. F., September 8, 1871, "he complained of a mist being constantly before his eyes, so that he was unable to recognize his most intimate friends above a yard off. I found that he could read slowly, and with effort, No. 20 of Snellen's test types at four inches from his eyes with the right eye, at eight inches with the left, and at six inches with both. On examination with the ophthalmoscope, the outer two-thirds of the optic disk in both eyes was found to be pearly white in tint and glistening, and the inner third hyperæmic, the retinal veins were enlarged and tortuous, and the arteries diminished in number and calibre. Alongside some of the vessels were to be seen the white lines said to be characteristic of *neuritis descendens*. There was here then a markedly atrophic condition of the optic nerve, most probably primary; there being no certain evidence either by the ophthalmoscope, or otherwise, of intra-ocular causes sufficient to produce such atrophy. I dismissed the idea of the disease being due to tobacco poisoning, although, in some respects the condition of the disk seemed closely allied to what is usually described as due to excess in smoking. At the same time there was not, and never had been, any symptom of cerebral affection. Still I have been strongly impressed with the idea that this was a case of primary degeneration of the optic nerve, a degeneration which may find illustration in cases of so-called tobacco amaurosis. Dr. F. advised

the patient to consult Dr. Thomas Reid, of Glasgow, who confirmed the diagnosis, and recommended the use of bichloride of mercury in combination with iodide of potassium, which treatment was commenced on the 10th of September, and continued for a month. During this time the patient was a good deal at the coast, and came back to town much improved in general health. His sight, however, was scarcely, if at all, improved; although he could read No. 20 at 7½ inches. At this visit, I passed a current from six cells of a Stöhrer's battery through the temples for about twenty seconds. On testing his sight immediately afterwards, I found that he could read No. 20 at 9½ inches; an improvement of two inches within a period of as many minutes. I advised him, however, to go on with the bichloride mixture for another week or so. I saw him again on the 8th October, when I found, as I expected, his temporary gain had left him, he being only able to see No. 20 at 7½ inches as before. I may here mention that in reading, my patient always sought to make the best of his case, so that the limit of clear definition would be more correctly stated at an inch less than the above figure. I again tested him with the current, the result being an improvement of three inches. Two days afterwards, he could read at 8½ inches. I again applied the current, and immediately afterwards he read at 10 inches. Feeling satisfied now that the galvanic treatment would yield good results, I asked him to cease taking the mixture, and to call upon me every morning at 10 A.M. In order to insure accuracy in the results, I kept him to the same hour, position as to light, &c. The days at this season, and at the above hour, were usually dark and foggy; conditions, by the way, in which he could see best. On clear, sunny days, he described the mist as being particularly thick and dark before his eyes. About a week after I began the galvanic treatment he improved in this respect, being able to see best on a clear day.

"I continued the galvanization daily for a month, then every second day, sometimes every third day, and again every day as I thought fit. I sought to avoid the dangers of over-stimulation by the weakness of the current—six cells—and the shortness of the application, which never exceeded thirty seconds. I applied the electrodes at first to the temples, and to the long axis of the head, at each break of the current producing the flash indicative of retinal irritation. Latterly, I applied one to the forehead, the other to the tongue, with alternations. Occasionally, and experimentally, I applied a current from eight cells to the cervical sympathetic—the results, however, never seemed so good as by the other methods.

"The treatment was continued for three months, during which time his progress was a matter of daily observation. At the end of that period he could read No. 5½ Snellen with as much ease as at the beginning he could read No. 20. The improvement in his health and spirits due to this progressive recovery of sight has been very marked." Dr. F. has tabulated the results, and it is shown that every day there was a distinct improvement of one inch in his ability to read the test types, and this improvement was confirmed by ophthalmoscopic examination. "On the 19th November, 1871," says Dr. F., "I fancied, on examining the fundus, that the condition of the disk was, if anything, slightly improved. On the 11th January, 1872, I made a very careful examination, and was satisfied that while the atrophic condition of the disk was still very marked, there was an improvement. This was particularly so in the case of the retinal circulation, the arteries being distinctly larger in calibre, while the veins were smaller and much less tortuous. As it was an important point that there should be no doubt as to this improvement, I again asked my patient to see Dr. Reid, who substantially agreed with me.

"The question now is, how far this improvement will go. I do not expect that much more can be effected in the case, although I mean to continue the treatment twice a week or so for some time to come. A more important question is, how far will the results be permanent? My impression is, that with care on the part of the patient his present condition may be maintained for a considerable length of time, if not permanently.

"But whether this be so or not, it is evident from the ophthalmoscopic examinations, that we have not simply stimulated the patient's retina, but that we have

distinctly improved its circulation, as well as the nutrition of the optic nerve, and that this has been done in a disease in which ordinary medical treatment is practically useless." [Dr. F. seems not to have read the paper of Prof. Nagel, of Tubingen, on the cure of white atrophy of optic nerve, related by Prof. Chisolm in our last number, page 59, successfully treated by hypodermic use of strychnia.] In an appendix to his paper, dated January 24, 1872, Dr. F. refers to a recent work, *Traité d'lEectricité Médicale, par les Docteurs E. Onimus et Ch. Legros*, Paris, 1872, where some cases of optic atrophy treated by galvanism are related.

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63. *Treatment of Conical Cornea by removal of the top of the Cone.*—Mr. C. BADER, of Guy's Hospital, reports (*Lancet*, Jan. 20, 1872) nine cases of conical cornea in which favourable results were obtained by removing the top of the cone.

The operation he describes as follows: 1. Place the patient on a bed as for extraction of cataract, and bring him thoroughly under chloroform.

2. Keep the eyelids open with a stop-speculum, so as to press upon the eyeball as little as possible, and fix the eyeball with the forceps.

3. Immediately before commencing the operation, ascertain the position of the top of the cone by turning the eye to be operated on in different directions, while strong light is thrown upon the cornea with a two-inch convex lens.

4. The subsequent steps of the operation, however they be taken, should tend to remove the top of the cone (the entire thickness of the cornea), so as to cut an opening through the cornea into the aqueous chamber. This opening, somewhat oval-shaped, is from one-twelfth to one-sixteenth of an inch in its longest diameter; it was measured immediately after removal of the apex of the cone, after escape of the aqueous humour, the cornea having collapsed and being in contact with the iris. The pupil, in all cases, was opposite the opening in the cornea; the surface of the lens could be seen bulging into the area of the pupil. In the cases in which the thread was used, the latter caused an indentation in the crystalline lens while passing across the area of the pupil.

5. *Removal of the Top of the Cone.*—A small curved needle, armed with fine white or black silk, or with silver wire (or a gilded, small, sharp hook), is thrust through the cornea in its horizontal diameter, close to the portion of cornea we wish to remove. The point of the needle, after piercing the cornea, is carried horizontally across the aqueous chamber to a spot opposite the point of entrance, and again thrust through the cornea close to the portion of cornea we intend to remove. The aqueous humour escapes before or after passing the point of the needle through the cornea the second time. The needle or sharp hook is left in the cornea until the top of the cone has been removed; it helps to protect and keep back the crystalline lens. The portion of cornea (the top of the cone) situated in front of the needle is then removed as best we can. This part of the operation is somewhat difficult, the cornea being transparent or nearly so, extremely thin and flaccid, and the iris and lens being in contact with the cornea. The head of the needle is held in one hand, and with a cataract-knife, or with a sharp, narrow, lancet-shaped knife, an incision is made through the cornea (a small flap incision, as in flap extraction for cataract). Having made the incision, the needle is let go, the small flap seized with an iris-forceps, and the rest of the cornea (of the cone) removed with the knife or with scissors.

6. *Closure of the Wound (opening) in the Cornea.*—If a sharp hook or a needle without thread has been used for transfixing the cone, it is withdrawn after removal of the cone, and the wound left open. The eyelids of both eyes are then closed, kept bound up and cool with wet lint, and the patient kept in bed for three days; on the third day the use of the eye not operated on is permitted, while the eye operated on is kept bound up until all redness has subsided. If a needle, armed with silk thread, has been used, the needle, after removal of the cone, is drawn out gently by the second opening in the cornea; so that the thread, while passing across the aqueous chamber, the surface of the iris, pupil, and crystalline lens, irritates those parts as little as possible. The opening in the cornea is closed by tying the thread, as is done when uniting

the margins of a wound by a suture. The suture is tied tightly. If it should give a little before completing the knot, it does not signify. The cornea, by the suture, is thrown into numerous folds. This folded condition continued in an extreme degree in one case for nearly four weeks. Having united the wound, one end of the thread is cut off close, the other left about a quarter of an inch long, so as to assist when removing the suture. Both eyes are kept cool and bound up with wet lint. The patient remains in bed. The suture is removed on the appearance of slight chemosis and swelling of the eyelids.

7. *Removal of the Suture.*—The patient being rendered insensible (by methylene), the lids are kept open without pressing upon the eyeball, the surface of the cornea is well cleared from mucus, &c., and the long end of the suture drawn away from the cornea, so as to stretch the suture, and the latter cut through and withdrawn. After this, the lids of both eyes are again carefully kept bound up with wet lint. The use of the eye not operated on is permitted on the third day after removal of the suture. The operated eye is kept bound up, and the lint cool, until all redness of the eyeball has subsided, or nearly so, when an artificial pupil is made.

8. Any other mode of destroying the apex of the cone in conical cornea—for instance, by the galvanic cautery—would, I believe, answer as well as abscission of the cone.

Disadvantages of the Operation.—An opaque spot in the cornea, from where the cone had been removed. The patients were told that there would, after the operation, remain a small white speck on the eye, but that nothing except an operation could improve sight. None of the patients complained of the disfigurement, and all were pleased with the amount of vision obtained.

The necessity of giving an anæsthetic repeatedly. For removal of the suture, and for the artificial pupil, methylene is quite sufficient.

Advantages of the Operation.—In extreme cases of conical cornea a greater improvement of sight is obtained than by any of the known modes of treatment.

No untoward accident occurred in any of the cases.

The after-treatment, after removal of the suture, is very simple. The patient need only be seen at great intervals.

The conical cornea in all cases completely disappeared, and gave way to an abnormally flat cornea.

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64. *Extraction of Cataract by a Peripheral Section of the Iris without injuring the Pupil.*—Dr. Charles Bell Taylor recommends (*Lancet*, Nov. 4, 1871) the following method: "Having separated the lids with the speculum, the eye should be gently turned downwards with a pair of ordinary forceps in the operator's right hand. Having got the globe into a favourable position, it should be fixed by the sharp forceps at about the junction of the upper with the middle third of the cornea; the pointed knife is then entered in the corneo-sclerotic junction one or two lines from the forceps at the summit of the cornea, pushed well into the anterior chamber, and then, with a gentle sawing motion, carried along the summit until about one-third of the cornea has been incised. The capsule is then carefully divided with Von Graefe's cystitome, having been previously rendered tense, and the eyeball fixed with a pair of ordinary forceps. (It is better to open the capsule at this stage, because bleeding from the wounded iris—and conjunctiva also—at a later period is apt to fill the chamber and render this part of the operation obscure and difficult.) The upper segment of the iris is then seized, and a small piece of the periphery only excised, the pupillary margin and portion of iris attached to it being left untouched and free in the anterior chamber; the lens is then extruded through the gap in the ordinary way, gliding behind the pupil, so that there is no stretching of the sphincter.

"In this way I believe that I have secured all the advantages, in the way of safety and certainty, of an associated iridectomy (which I have already detailed), and at the same time attained that grand desideratum—a central and movable pupil.

"The instruments I employ are a pair of sharp forceps that pierce the sclerotic; a very light speculum (a modification of Von Graefe's); and two knives,

a line in width, and bent at an angle similar to the ordinary iridectomy knife—one with a sharp point, the other with a blunt or bulbous extremity."

65. *Iridectomy without Division of the Sphincter Pupillæ.*—In a communication from Dr. B. A. POPE, contained in the *Archiv f. Augen u. Ohren Heilkunde II*, he suggests, that, when iridectomy is demanded simply for an optical purpose, the operation may be so performed as to avoid division of the sphincter of the pupil, and thus to prevent any injury to the natural movements of the proper pupil, which remains entirely distinct from the artificial one. To this intent Dr. P. has already operated with good results in six cases. His plan is, after an incision of the cornea, to allow the fluid of the anterior chamber to escape gradually, so as to prevent a prolapse of the iris. The latter is now to be seized by a small optical forceps at its pupillar edge, at the point where it lies beneath the most transparent part of the cornea, and gently drawn towards the centre of the eye, with the scissors a fold of the iris lying between the point of the forceps and the edge of the cornea is to be removed. The ordinary operation for iridectomy Dr. P. restricts to those cases in which from it is anticipated simply a therapeutical result, or both a therapeutical and optical result.—*Centralblatt f. d. Med. Wissenschaften*, Nov. 12, 1871, No. 46.

D. F. C.

66. *Paralysis of Ocular Muscles treated by Calabar Bean.*—Mr. T. WHARTON JONES reports (*Practitioner*, December, 1871) two cases of paralysis, one of the superior oblique muscle, the other of the third nerve, both successfully treated by dropping on the eye a solution of the extract of Calabar bean.

67. *Effects of Calabar Bean and Atropia on the Human Eye after Death.*—BORELLI found that, if applied to the eye within six hours after death, these substances act on the pupil as during life. In exceptional cases, effects followed their application so long as twenty-four hours after death. He found the result of their application less marked in cases where death had ensued from old age or marasmus. The length of time that had elapsed after death also influenced the result. A reflection of the myotic action of the Calabar bean from the one eye to the other was also noticed, which never was seen in the case of the mydriatic action of atropia. This difference Borelli attributes to the fact that the brain, which is the organ of reflex action for the circular fibres of the iris, preserves its vitality longer after death than the great sympathetic which innervates the radiating fibres.—*Edin. Med. Journ.*, Nov. 1871, from *Giornale d'Oftalmologia Italiano.*

MIDWIFERY AND GYNÆCOLOGY.

68. *Concurrent Impregnation of both sides of a Double Uterus.*—Dr. J. HARRIS ROSS records (*Lancet*, Aug. 5, 1871) an extremely interesting case of this. The subject of it was thirty-eight years of age, had had six children, nothing particular was noticed with regard to her confinements. July 16, 1870, she miscarried of twins, apparently between the fifth and sixth month. During the labour a second opening was felt close to the os uteri, but its precise nature was not then made out. About a week after the labour she declared that she was then pregnant, and the presence of another child was made out. She was delivered of a healthy female child on October 31, fifteen weeks and two days after miscarrying with twins. After recovery a complete examination was made. Two openings were found in front of the cervix. Upon introducing the sound into the upper or right aperture, it passed quite freely into a cavity two inches and a half deep to the summit, where it was cautiously moved in every direction with a view of discovering any communication that might exist between this and the supposed adjoining cavity, and on withdrawing the instru-

ment the same caution was exercised, and for the same purpose, but no such outlet or passage could be detected. The sound was then passed into the lower or left aperture, and traversed quite easily a cavity two inches and a quarter deep only, nearly parallel to the preceding cavity. Thus, between the two cavities there was a vertical partition or septum extending from the fundus to the front of the cervix uteri, and completely dividing the latter into two separate or distinct cavities. A curious corroborative proof of the concurrent impregnation in two distinct uterine cavities was, that the patient had menstruated (evidently from the uterus which had thrown off its contents) three times between her miscarriage in July and her delivery in October.

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69. *Premature Delivery induced to Prevent the Intra-uterine Death of the Fœtus.*—Prof. A. Godefroy, of Rennes, records (*Revue de Thérapeutique*, Dec. 15, 1871) the case of a lady, who at three successive pregnancies lost her children at the eighth month of gestation. As both father and mother were free from any syphilitic taint, Prof. G., and the lady's attendant, Dr. Baudouin, were led to ascribe the death of the fœtuses to the anæmic condition of the mother which disabled her from nourishing her fœtus till full term. When she became pregnant for the fourth time, it was determined to induce labour towards the end of the eighth month, which was accordingly successfully accomplished and a child delivered. A wet-nurse had been provided, and the infant with great care was raised. The mother recovered as well as from her previous labours, and the tonic regimen which had been previously prescribed was ordered to be continued.

This method of treatment was twice practised by Denman with success, for both mother and children, and once with a like happy result by Hayn of Kœnigsberg.

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70. *Inversion of the Uterus.*—Dr. John Thompson, of Bideford, relates (*British Med. Journ.*, Dec. 16, 1871) a very interesting case of this. The subject of it was a farmer's wife who had been previously confined five times. There had been some difficulty in all these labours, either a faulty presentation, or retained placenta, or the birth was premature. In her sixth labour the presentation was natural, she had arrived at full time, and delivery took place in a few hours. After handing the infant to the nurse, Dr. T. applied his hand to the abdomen of the mother, but instead of the usual uterine tumour there was, he says, "a vacuity such as I never before met with. As I had not attempted to withdraw the after-birth, this seemed strange; nevertheless, believing that inversion of the uterus only occurred where traction had been exercised on the funis, it did not strike me that it could have happened here. On examining the vagina, I found the placenta, and at once attempted its removal. In a few seconds it came away with an unusual sort of plunge as if a coagulum had emerged with it, and I tried to remove the mass from under the bedclothes to the ordinary receptacle, but found myself hindered by a band which appeared stouter than the ordinary membrane, and which was, in fact, the inverted vagina. Lifting the bedclothes, I was astonished to observe the after-birth and womb both escaped together, the latter completely inverted, and having the placenta partially attached. There was no bleeding, nor had much blood come away with the vagina. Promptly taking off my coat, and baring my arm, I first peeled off the placenta—this was done with great facility. No blood flowed from the uterine surface. Then, taking the uterus in my right hand, I passed it up the vagina, and, bending my fist, pressed nimbly against the fundus (my left hand meanwhile supporting the abdomen) and in an instant restored its position. My right hand passed into the uterine cavity, where I allowed it to remain till contraction came on, and it was only withdrawn when uterine action became decidedly expulsive. The patient, during this time, experienced no shock in her system; she lost but little blood, and subsequently made as good a recovery as she had done in any former confinement. Since that time she has not been pregnant, though she has had good general health, and looks ruddy, vigorous, and cheerful.

Two practical views derive support from the facts of this case; the first, that inversion may, and sometimes does, take place without interference on the part

of the attendant; the second, that reinversion may be readily effected, if the manipulation be prompt and well directed. Formerly, it seemed to be the opinion of the profession that inversion of the uterus always indicated a faulty interference on the part of the practitioner—perhaps the view now generally held is that this is only the most frequent cause." * * * "The ease with which I restored the displacement was the most remarkable feature in the case; it contrasts strikingly with the distressing failures related in the columns of the different medical journals. The reason for this difference seems to me clearly attributable to the fact of my having acted at once. It is noticeable that in nearly every unsuccessful case the accident has occurred for some time before attempts have been made at reduction—in the majority it could not then be remedied. A reason for delay to procure professional assistance is afforded when, as in the instance I have described, the mouth of the uterus contracts over the course of the uterine vessels and prevents hemorrhage, but it is to be remembered that this is the very case likely to give the most resistance to reduction if delay be allowed."

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71. *Treatment of Dysmenorrhœa resulting from Abnormal Constriction of the Uterine Canal.*—Dr. PROTHEROE SMITH recommends (*Brit. Med. Journ.*, Dec. 16, 1871) the following method of treating this form of dysmenorrhœa: First he prepares the patient "by a purgative dose, and by abstinence from local excitement, and from alcoholic drinks, or much animal food. When hyperæmia exists, I scarify laterally the labia uteri repeatedly at the commissures of the labia, by which the vascularity of the organ is reduced, and the shape of the os tincæ, when constricted, is improved. After accustoming the uterine canal to bear a metal bougie, which should be repeatedly and daily introduced, and increased in size until that of a No. 10 catheter can be borne without any pain, then the uterine dilator may safely be employed." [This instrument is constructed after the model of Huerteloup's lithotrite, by which the extent and direction of the uterine cavity can be measured and a constriction of any part of the passage readily overcome.] It consists of "two short blades, two and a half inches long, the inner being continuous with the sliding shaft, with which it is nearly at a right angle, having at its proximate end a screw worked by a nut so as to mark precisely, by an index on the handle, the extent of dilatation employed. This should be used at first cautiously about every second day, always ceasing to screw as soon as pain is experienced. This is immediately relieved by a turn or two of the screw the reverse way.

"It will be found, in a short time, that the uterus becomes accustomed to the dilatation, when it may be employed to a greater extent; and in the course of a few days or weeks, as the case may be, a forced dilatation to the extent of an inch or an inch and a half may be used with impunity. After this it will only be necessary to use the dilator daily for two or three days, and afterwards at longer intervals, to keep the parts open till they permanently heal in the state of distension effected by the operation.

"Should any congestion or inflammation result, scarifying at the commissures of the labia will relieve by free bleeding, whilst, at the same time, the os tincæ is made to assume a more open, and, therefore, a more normal shape. I prefer, generally, to effect this by the repeated use of a small scimetar-shaped knife, as I find that by so doing the risk of inflammation is diminished, and it prevents cohesion of the cut sides of the labia uteri, both which accidents occasionally attend the operation when performed at once by a hysteratome. When preternatural shortness of the uterus, from original malformation, exists, the operation is contraindicated; also when stricture depends on endometritis; when, otherwise, metritis or metrocellulitis is present; when there are fibroid tumours causing inflammatory adhesions; when dysmenorrhœa is characterized by deciduous membranes; when there are conical hypertrophy and elongation of the cervix; when there is globular enlargement of the anterior labium uteri, embraced by the posterior lip in the form of a crescentic membrane; and when displacements and dislocations of the organ complicate the case—these and all other organic diseases which may attend this malady should be removed prior to the adoption of forcible extension by the dilator."

72. *Treatment of Ovaritis.*—Dr. J. Matthews Duncan divides ovaritis into acute and chronic. The acute form may end simply by resolution, or its termination may be complicated by perioophoric adhesions or abscess, or true ovarian abscess, or it may end in the chronic form of the disease. Chronic ovaritis may last for many years without the organ becoming fixed by adhesions, and without suppuration in its substance or in its immediate neighbourhood. One ovary only may be affected or both sides, and the disease may be limited to one or attack both alternately. The left ovary, he thinks he has observed, is more frequently the seat of disease than the right. He has seen it enlarged to at least three times its ordinary dimensions. The symptoms cognizable by the practitioner are sensitiveness, tenderness, degrees of hardness, enlargement, roughness of surface, and change of position. Ovaritis is frequently caused by the conditions of recent marriage, or may be produced by suppression of the menses from cold or other causes; it is often observed as a consequence of gonorrhœa; it occurs frequently during convalescence from abortion. It is less common after delivery at the full time. It is observed frequently after operations on the uterus, such as metrotomy and other dilatations of the cervix, and it is frequently found in cases where no evident cause can be assigned to it. Ovaritis occasions pain, which may be either slight or severe, acute or dull, in the region of the ovary, groins, back, sacrum, or down the thighs. It is not necessarily accompanied by suppression, or even any diminution, of the lochia or of the catamenial flow, nor by menorrhagia, though these symptoms may occur. Dr. Duncan does not believe that ovaritis is inconsistent with fertility, though it no doubt is a frequent cause of sterility. A woman suffering from ovaritis, acute or chronic, can rarely submit to sexual connection, on account of the pain it inflicts. Patients suffering from ovaritis often quickly assume evident outward appearances of depraved health, the dull eye, the pasty face, pallor, and anæmic look. Ovaritis is only to be made out exactly by a physical examination, the details of which are fully given in Dr. Duncan's paper. The prognosis should always be very guarded; for although many cases mend rapidly, many are very tedious. In the treatment the invaluable condition of rest of the affected organ cannot be maintained, the ovarian congestion attendant upon the maturation and bursting of a Graafian follicle coming to undo all that treatment may have effected. In chronic cases two, three, or four leeches may be applied through a glass speculum to the cervix uteri, and the bleeding from the leech-bites encouraged, if need be, by hot fomentation to the vulva. In some cases it may be preferred to apply a larger number of leeches over the inguinal canal. As in other chronic inflammations counter-irritation is here often useful. It is best effected by applying the irritant over the inguinal canal adjacent to the affected gland. A small extent of counter-irritation, say about two inches square, is sufficient. It may be produced by a croton oil liniment or antimonial ointment, or by keeping a blistered surface from healing. Instead of these forms of counter-irritation a seton may be used. The regulation of the bowels is important, and for this purpose the gentle salines are best adapted. Some patients derive advantage from the use of mineral waters, such as those of Kreuznach; others receive benefit from the waters of Homburg or of Kissingen. Iodine, bromine, and mercury may be used with all the care that is exercised in the administration of these remedies; in other chronic inflammations Dr. Duncan does not think the arrestment of leucorrhœa, especially by speculum and caustic, desirable, at least in an early part of the course of a case, and he has little doubt that this kind of treatment is occasionally a cause of ovaritis.—*Practitioner*, Dec. 1871, from *Edin. Med. Journ.*, Sep. 1871.

MEDICAL JURISPRUDENCE AND TOXICOLOGY.

73. *Toxical Studies on Chloral Hydrate*, by B. W. RICHARDSON, M.D., F.R.S. —The most important subject, in a toxicological point of view, which has recently been under discussion, has relation to chloral hydrate. The increasing employment of this narcotic by the members of the general public, without the advice of the professors of medical science, has been attended by several fatal occurrences; while in instances where fatal results have not taken place, peculiar symptoms have followed the frequent self-administration of the narcotic—symptoms of serious import, and demanding ready recognition. We have, consequently, devoted some labour to the investigation of certain facts bearing on the toxicological history of chloral hydrate, to which, and the results of our inquiries, we would now direct attention.

1. We have endeavoured to ascertain what is a dangerous and what a fatal dose of chloral hydrate. The conclusion at which we have been able first to arrive on this point is, that the maximum quantity of the hydrate that can be borne, at one dose, bears some proportion to the weight of the animal subjected to its influence. The rule, however, does not extend equally to animals of any and every class. The proportion is practically the same in the same classes, but there is no actual universality of rule. A mouse weighing from three-quarters of an ounce to an ounce will be put to sleep by one-quarter of a grain of the hydrate, and will be killed by a grain. A pigeon weighing twelve ounces will be put to sleep by two grains of the hydrate, and will be killed by five grains. A guineapig weighing sixteen ounces will be put by two grains into deep sleep, and by five grains into fatal sleep. A rabbit weighing eighty-eight ounces will be thrown by thirty grains into deep sleep, and by sixty grains into fatal sleep.

The human subject, weighing from one hundred and twenty to one hundred and forty pounds, will be made by ninety grains to pass into deep sleep, and by one hundred and forty grains into a sleep that will be dangerous.

From the effects produced on a man who had of his own accord taken a hundred and twenty grains of the hydrate, and who seemed at one period to be passing into death, we were led to infer that in the human subject one hundred and forty grains should be accepted as dangerous, and one hundred and eighty as a fatal dose. Evidence has, however, recently been brought before us which leads us to think that, although one hundred and eighty grains would in most instances prove fatal, it could, under very favourable circumstances, be recovered from.

Dr. Hills, of the Thorpe Asylum, Norwich, has, for example, favoured us with the facts of an instance in which a suicidal woman took no less than *four hundred and seventy-two grains* of the hydrate dissolved in sixteen ounces of water, and actually did not die for thirty-three hours. Such a fact, ably observed as it was, is startling; but it does not, we think, militate against the rule that one-hundred and forty grains is the maximum quantity that should, under any circumstances, be administered to the human subject.

2. A second point to which our attention has been directed is, what quantity of hydrate of chloral can be taken with safety at given intervals for a given period of time, say of twenty-four hours. To arrive at some fair conclusion on this subject, we calculated from a series of experiments the time required for the development of symptoms from different doses of the hydrate, the full period of the symptoms, and the time when they had entirely passed away. Great difficulties attend this line of investigation; but we may state, as a near approximation to the truth, that an adult person who has taken chloral hydrate in sufficient quantity to be influenced by it, disposes of it at the rate of about seven grains per hour. In repeated doses, the hydrate of chloral might therefore be given at the rate of twelve grains every two hours for twenty-four hours, with less danger than would occur from giving twelve times twelve (144) grains at once; but we do not think that amount ought, except in the extremest emergencies, to be exceeded, in divided quantities.

3. A third point to which we have paid attention is, the means to be adopted in any case when, from accident or other cause, a large and fatal dose of chloral hydrate has been administered. We can speak here with precision. It should be remembered that this hydrate, from its great solubility, is rapidly diffused through all the organism. It is in vain, consequently, to attempt its removal by any extreme measures after it has fairly taken effect. In other words, the animal or person under chloral, like an animal or person in a fever, must go through a distinct series of stages on the way to recovery or death; and these stages will be long or short, slightly dangerous or intensely dangerous, all but fatal or actually fatal, according to the conditions by which the animal is surrounded. One of the first and marked effects of the chloral is reduction of the animal temperature; and when an animal is deeply under the influence of the agent, in the fourth degree of narcotism of Dr. Snow, the temperature of its body, unless the external warmth be carefully sustained, will quickly descend seven and eight degrees below the natural standard. Such reduction of temperature is itself a source of danger; it allows condensation of fluid on the bronchial pulmonary surface, and so induces apnœa, and it indicates a period when the convulsion of cold (a convulsion which sharply precedes death) is at hand.

We offer these explanations in order to indicate the first favourable condition for the recovery of an animal or man from the effects of an extreme dose of chloral hydrate. It is essential that the body of the animal be kept warm, and not merely so, but that the air inspired by the animal be of high temperature. The first effort to recovery, in short, should consist in placing the animal in a warm air. This fact is perfectly illustrated by experiment on the inferior animals. In the pigeon an air of 95° Fahr. is most favourable, in the rabbit an air at 105° to 110°, in the dog the same. In man the air to be breathed should be raised to and sustained at 90° Fahr. at least.[1]

The next thing to be remembered in the recovery of persons under the fatal influence of chloral hydrate is to sustain the body by food. I find that even under deep sleep from the narcotic, although the process of waste is less than is common under natural conditions of rest, there is still a very considerable waste in progress, which, if not made up, is against recovery. I find also that the digestive and assimilating powers, though impaired during sleep from chloral, are not arrested, but may be called into fair action with so much advantage, that if two animals be cast into deep sleep by an excessive quantity of the narcotic, and one be left without food and the other be artificially fed on warm food, one-fourth of the chance of recovery is given to the animal that is supplied with food. In the human subject warm milk, to which a little lime-water has been added, is the best food. Milk is very easily administered mechanically, and it should be administered in the proportion of half a pint every two hours.

4. The fourth point to remember is to sustain the breathing; in the inferior animals the question of life or death can be made to turn on this pivot. But the artificial respiration must be carried out with great gentleness; it must not be done by vehement movements of the body or compressions of the chest, but by the simple process of inflating the lungs by means of small bellows, through the nostrils. We have devised in the course of our researches various instruments for artificial respiration, viz., a small double-acting bellows, a small syringe, and a double-acting India-rubber pocket-bellows; and we have lately made an instrument which acts by a simpler method still, *i. e.* we merely attach to a single hand-bellows a nostril-tube, and gently inflate the lungs, letting the elasticity of the chest-wall do the work of expiration. A little valve near to the nostril tube effectually stops all back currents from the lungs into the bellows. For the human subject, five charges of air from the bellows should be given at intervals of five seconds apart.

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[1] We have no doubt it will be found, as the chronicle of deaths from chloral hydrate increases, that the mortality from the agent will be the greatest when the thermometrical readings are the lowest, and *vice versâ*.

The symptoms of acute poisoning by chloral hydrate are, briefly, profound coma, great muscular relaxation, apoplectic breathing, and flushing of the face and neck, with intermissions of pallor. The eyes are usually rolled upwards as under chloroform, and at times, as impressions of motion are made on the surface of the body, there is muscular tremor, which may pass into convulsion. In time the extremities become cold, and the bronchial surface becomes charged with frothy mucus, which greatly impedes the breathing and hastens the final result.

The chronic symptoms of chloral poisoning are—sleeplessness, unless the narcotic be taken in very large doses; great mental irritability and muscular prostration; uncertainty of movement, with tendency to fall forward; caprice of appetite and frequent nausea. In some cases there is injection of the conjunctivæ, and in other cases yellowness. The urine in extreme cases contains albumen, and the bowels are commonly constipated, the evacuations being white and hard. Chloral hydrate does not produce the ecstatic dream or delirium caused by opium or haschish; on the contrary it causes, through all the stages of its action, a sense rather of depression than of elevation of mental faculty.—*Brit. and For. Medico-Chirurgical Rev.*, Jan. 1872.

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74. *Recovery after Swallowing One Hundred Grains of Chloral.*—Mr. B. BROWNING records (*Brit. Med. Journal*, Dec. 2, 1871) the following case of this: An anæmic middle-aged woman while suffering from facial neuralgia, swallowed at one draught a recently prepared syrupy solution of chloral hydrate, which contained over 100 grains of that drug. She immediately complained of "intense burning pain in the throat, gullet, and stomach;" and when I saw her (about three-quarters of an hour subsequently), she was screaming and almost convulsed with agony—so much so, that it was with the greatest difficulty she could be restrained from throwing herself out of her bedroom-window into the road. Her pulse was small and very rapid; the countenance was livid and bathed in sweat; an odour of chloroform was plainly perceptible in the breath, and no trace of narcotism had been observed, even for a moment. After an ineffectual attempt to bring on vomiting, I injected hypodermically half a grain of morphia over the epigastrium, and directed milk to be freely given. On visiting her in three hours' time, I found her somewhat relieved, and accordingly injected another half grain of morphia, and suggested the cautious use of stimulants with the milk. On the following morning she was free from all pain of any description, though very weak, and has since done well; a slight return of neuralgia being at once checked by another employment of the hypodermic syringe and ferruginous tonics.

The usual hypnotic effects of the chloral were here "conspicuous by their absence" from first to last; no drowsiness even, much less sleep, having set in during the period of its elimination from the system; yet in a previous neuralgic attack, twenty grains only had procured a good night's rest for this person. I can vouch for the amount, as well as the goodness, of the chloral taken by her; and think that, as deaths have been reported after the administration of 60, 45, and even 25, grains of this remedy, she may be deemed fortunate in escaping so readily.

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75. *Poisoning by Strychnia successfully treated by Chloroform Inhalations.*—We find in *Le Mouvement Médical* (Dec. 3, 1871), a report of a case, taken from the *Gaz. Med. de Torino*, in which a man took with suicidal intent three grains of strychnia. Violent convulsions followed, which were relieved by the inhalation of chloroform. Whenever this was stopped the convulsions recurred. By persisting in this treatment the patient entirely recovered.

AMERICAN INTELLIGENCE.

ORIGINAL COMMUNICATIONS.

Malignant Hemorrhagic Smallpox; Recovery. By Wm. T. Taylor, M.D., of Philadelphia.

Dr. Collie remarks (*Lancet*, Dec. 1871) that "this form of the disease is invariably fatal," and such is the general opinion, but the following case proved an exception.

Mrs. R. E., æt. 39, the mother of four children, was taken sick December 10, 1871, with soreness over the whole body, headache, and great pain in the small of the back, attended with sick stomach and frequent vomiting.

13*th*. Pain in back abated and an eruption resembling "prickly heat" appeared on the face and arms which had extended over the body by the next morning, when I was called in. She then had fever, headache, redness of the conjunctiva, sore throat, and great thirst. Having taken a dose of castor oil on the previous day, her bowels had been moved freely, and the stools were mixed with blood. She was nursing an infant eight months old, unvaccinated. She had been vaccinated in infancy, but her vaccine scar was scarcely perceptible. The babe was removed and vaccinated. As the sanitary condition of the house was unfavourable, and she could not be properly cared for at home, I advised her removal to the hospital, but she and her husband refused. I ordered her a tablespoonful every two hours of the following mixture: Potas. citras ʒij; spt. nit. dulc. ʒij; tinct. opii comp., elix. ammon. valer., aa ℥ss; liq. limonis ʒij; aq. camphor. ℥ij.

15*th*. Papules changed into vesicles on the face and arms. As she was very thirsty, I ordered milk with eggs, sweetened, to be used freely, but she took them very sparingly, complaining that the milk filled her throat with slime and caused her to be constantly expectorating. On the following day her throat became very sore, and she could barely swallow milk, which she refused unless largely diluted. Her face began to swell, and the eyelids soon closed. She expectorated bloody mucus, and her stools were frequent, being tinged with blood.

17*th*. Vesicles flat, filling with blood, extending over the whole body, from the size of a pea and smaller to some one inch in diameter, whilst on the hands there are large bloody blebs. I ordered brandy and milk, with beef-essence, and a teaspoonful every two hours of the following mixture: Quin. sulph. gr. xij; tinct. ferri chl. ʒss; syrupii ℥j; aq. menth. pip. ℥ss. Of this she took two doses and then refused to take more.

18*th*. Wildly delirious, requires constant watching to keep her in bed; once during a momentary absence of her husband she attempted to get out of the window. Much oppressed in her breathing; voice husky; throat and fauces dry, so that she can scarcely swallow.

19*th*. Face still continues swollen; tongue dry; blood spots cover the body; constant jactitation. Refused the milk-punch, but will swallow a little brandy and water. Her condition appears hopeless.

20*th*. Condition unchanged; still delirious; passes her urine in the bed. At midnight was so greatly prostrated that her death was expected at any moment, but I was greatly surprised to find her living the next morning, and although the purpuric spots covered the whole surface of the body,

yet they were beginning to dry on the face, and the swelling was subsiding. To-day she took some coffee and milk with brandy and water.

22*d*. She recognized me this morning and appeared quite rational, answering my questions intelligently, saying she "feels good." Desiccation is going on rapidly over the face and arms, but the purpuric character of the disease is still apparent over the body and legs. She takes brandy and water and soup. Drs. Thos. J. Yarrow and Emory Eshleman, who saw the patient with me to-day at my request, freely corroborate my views as to its malignant hemorrhagic character, and the unfavourable condition of the house.

25*th*. Eruption drying rapidly on the face and is desquamating, but the skin is peeling from the hands. Has a slight cough and hoarseness, which yielded readily to mist. glycyrrh. comp. Appetite good, and she took some chicken soup.

27*th*. Brown appearance of spots still apparent on the legs and body, resembling "blood-blisters" when dry. Desquamation going on rapidly, but patient complains of sore throat, which was relieved by an infusion of sumac berries and chlorate of potash. As she feels weak, I ordered her a teaspoonful three times a day of the mixture of quinia and iron which had been previously ordered and not administered.

January 1, 1872. Is now perfectly well and has returned to her domestic duties, having really escaped from the "jaws of death." All the family had been vaccinated, and not one of them was affected with smallpox.

Case of Monstrosity. By B. SCHERMERHORN, M.D., of Buskirk's Bridge, New York.

I was called, January 31, 1872, to see Mrs. P., mother of nine healthy children, said to be in labour. I had attended her before, and she had told me that she had always been delivered before the doctor's arrival. This I found to be the case on the present occasion. The child was born just as I drove up to the house. I immediately tied and cut the cord, and discovered that it was not a perfect child. After attending properly to the mother, on examination of the child I found it to be a female, weighing about four pounds; deformed, having but one eye, situated in the space between where the two eyes usually are; there was no nose; there was a pendant growth about an inch in length and an inch and one-half in circumference, attached to the forehead immediately above and overhanging the eye. There were also two similar but smaller growths attached to the little finger of each hand. In other respects the child seemed well formed. It never, however, made any crying sound; neither could I discover by the hand laid over the præcardial region that the heart beat.

DOMESTIC SUMMARY.

Menstrual Pelvi-Peritonitis.—Dr. THEOPHILUS PARVIN relates (*American Practitioner*, Dec. 1871) two interesting cases of this disease, and adds the following remarks on its diagnosis and therapeutics:—

"The *diagnosis* of menstrual pelvi-peritonitis will depend, in the first place, upon some obvious menstrual disorder. This disorder existing, we determine simply the etiology of the affection, and further investigations will be into the symptoms of the inflammatory affection in general. *Pain* is the most salient phenomenon of peritonitis. 'This cry of alarm' is in serous membranes highly characteristic.

"A patient with pleuritis describes his sufferings as if a sharp knife were thrust into his side. He hesitates to move, to cough, to take a full inspiration, to do anything which will put the intercostal muscles of the affected side in action. So, too, a woman with pelvi-peritonitis will complain of a pain similarly intense and sharp. Instinct insists not only upon general rest, but also upon local rest, and she lies down on her back, with the limbs drawn up, to relax the abdominal muscles. The pain is in the hypogastric, or in one or both iliac regions, or in all three, and often irradiates over the abdomen, or extends down the inner and upper portion of one or both limbs. Conjoined with pain are tenderness on pressure, and a sensation of fulness or distension in the lower part of the abdomen. Often there is great irritability of the bladder, and micturition is not only frequent, but difficult and painful. Comparatively seldom the rectum is disturbed; though later on in the disease, if the tumour formed should be retro-uterine, it may be very irritable. Nausea and vomiting quite commonly mark the onset of the disease; and as the latter is frequently of 'bilious' matter, and as there may be pain in the right side extending to the liver, and as there is fever, often preceded by a slight chill or chilliness, the disorder is at first sometimes mistaken in malarial regions for an attack of remittent fever.

"The formation of a tumour or tumours in one or more of the vaginal cul-de-sacs is a marked characteristic of this form of inflammation. The tumour may generally be found within a week after the onset of the disease, and before this the sac which is to be invaded by this swelling will be more sensitive to pressure, and less elastic than natural. 'This sensation of a tumour, which is recognized in one or more of the vaginal cul-de-sacs, is so much more interesting to study, as this sign in pelvi-peritonitis is the analogue of the dulness, or rather the analogue of the deficiency of elasticity on percussion which is found in pleurisy, and as the dulness furnishes one of the most important elements of diagnosis. . . . . The tumour is in juxtaposition to the uterus, not part of it, and thus is distinguished from enlargements, partial or general, which that organ may have. It is separated from it by a furrow or sulcus, sometimes very distinct, at others slight; and then it is by the difference of level, of consistence, and of elasticity, and by its special configuration, that it is to be distinguished.' (Bernutz.)

"Another point to be observed in making a diagnosis is the appreciation of 'the displacements, versions or flexions, and rotations which the uterus has undergone,' consequent upon the tumour or tumours encroaching upon its normal position, or, further on in the progress of the disease, from inflammatory adhesions.

"Finally, not to prolong the consideration of this topic, an exacerbation of inflammation, a '*recrudescence*,' is no uncommon event in pelvi-peritonitis at the accession of the usual monthly periods, especially if the menstrual flow either fails to appear or is scanty.

"In the therapeutics of this disorder few remedies are required, yet these used intelligently are wonderfully efficient. The first of these I would mention is local depletion by leeches. Where it is possible, the leeches should be applied directly to the neck of the womb; but when the vagina is so swollen or sensitive that a speculum cannot well be introduced, then their application should be made to the lower part of the abdomen, and at least three times as many should be used; for Bernutz justly observes that four leeches to the cervix accomplish more in depleting the inflamed part than a dozen used externally. The leeching may be repeated on the second and on the third day, if there has been no material improvement in the disease. About the third or fourth day a large camphorated blister,[1] occupying at least a third more than the sensitive portion of the abdomen, will be found useful if the symptoms are not yielding

[1] Smaller blisters upon one or the other iliac regions, as may be indicated, the blistered surface being dressed with morphia, will subsequently be found useful to dissipate the cruel neuralgic pains which in many cases are present after all inflammatory symptoms have subsided. So, too, small blisters similarly applied are useful in hastening the resolution of the tumour when this process seems to be slow or arrested.

readily to the previous depletion. Internally, no remedy is so important as opium. Indeed many cases of pelvi-peritonitis will make good recoveries under it alone. I am in the habit of giving the opium, in combination with quinia and the extract of conium, in the proportions previously mentioned. No one need withhold it for fear of constipating the bowels. This should be desired rather than feared. There are no "peccant humours" to be purged away, and *rest* for the whole body, the intestinal canal included, is a most important element in the successful treatment of the disease. Once in four or five days is often enough for an evacuation from the bowels; and when this does not occur spontaneously, a copious injection of warm water, especially when thrown high up in the bowel by means of a long tube, will generally have the desired effect. If any laxative is administered by the mouth, it should be one of the milder salines, and then only as an efficient preparation for the better action of the enema, which should succeed it in three or four hours. At the approach of a monthly period efforts should be made to secure a free discharge. These failing, when the menstrual *molimen* is manifest, two or three leeches to the neck of the womb will be of great advantage.

"Warm hip-baths after the acute stage of the disease has passed are often exceedingly agreeable to the patient, and are of some value in lessening the long-lingering soreness in the lower part of the abdomen.

"Of course, too, once this acute stage is passed, the practitioner will give suitable attention to any condition of the womb which may have been the cause of the menstrual disorder. An endo-metritis, a flexion, narrowing of the cervical canal, etc., may require to be cured before the patient can be assured against similar attacks at other monthly periods.

"It is important that the patient should be warned against an early resumption of her ordinary avocations. Better too prolonged rest than too early exercise. Probably no well-marked case of pelvi-peritonitis recovers entirely, under the most favourable circumstances, in less than six weeks or two months. The *abdominal corset* previously mentioned, or some similar bandage, will be found in many cases of great advantage, enabling patients to sit up and walk without discomfort, when not wearing it their suffering would be acute at every movement or jar. Whether, as Bernutz teaches, his bandage *immobilizes* the uterus, or whether it lifts upward and backward the abdominal viscera, so as to prevent so much pressure upon the recently-inflamed pelvic peritoneum, there can be no question as to its great utility.

"It will be observed that in the enumeration of therapeutic agents in this disorder no mention has been made of mercurials. I believe that calomel, or any other mercurial, administered as a cathartic, is decidedly injurious, and that the supposed antiphlogistic action of small doses is unnecessary. Opium is enough."

The Length of Time an Ovum may be retained after Death of the Fœtus.—Dr. JACOBI exhibited to the New York Obstetrical Society, March 7, 1871, a specimen showing the length of time an ovum may be retained in utero after the death of the fœtus. Last December (1870) Dr. Guleke was called to see a woman who said she was five months advanced in pregnancy—she ought now to be in the eighth month—but forty-eight hours ago, after the usual labour-pains, she expelled an entire ovum, much shrunken in appearance. The placenta was pretty well formed, but in a state of fatty degeneration. It was evident from the appearance of the surface of the placenta, that its attachment to the uterine wall had but recently been disrupted. The fœtus was apparently but ten or eleven weeks old. The ovum was retained about five and a half months after the death of the fœtus. No hemorrhage followed the expulsion of the mass. Dr. Jacobi said the longest time he had ever seen an ovum of this size retained after death of the fœtus was seven months. He now has a lady under observation, who, when at the third month, was taken with flooding; on examination he could feel the ovum, but made no attempt to remove it, as he considered it to act as a plug preventing further hemorrhage; the os closed, and the cervix resumed very nearly its former length, though it was a little softer and larger than in the unimpregnated state; the uterus is now slightly anteflexed, the ovum is still retained, and the uterus has not in-

creased in size during the last three months; the woman is in good health and does not menstruate.

Dr. NOEGGERATH said it not unfrequently occurs that the entire ovum may be retained a number of months after death of the fœtus, though more frequently a part of the afterbirth or decidua is retained. He has seen two instances which show how long a part of the placenta may remain without being detached. He is now treating a lady who aborted six years ago at the third or fourth month. The physician in attendance thought the ovum did not come away entire, the patient had considerable flooding at the time and made a slow recovery, and when menstruation became re-established she suffered from severe menorrhagia. Some time after she again became pregnant, went to full term, and was safely delivered, the secundines being removed entire. After getting up she still thought she had uterine trouble, as she suffered from bearing-down pains, menorrhagia, and metrorrhagia. Her physician advised her to go to Europe, where she was under the care of a prominent gynæcologist in Berlin, who said a latero-version existed, as also a granular erosion of the cervix uteri, which condition he considered sufficient to account for her hemorrhage, anæmia, etc. While in Berlin she wore a stem pessary, with some relief to her symptoms. On her return to this country she again had severe metrorrhagia and sent for Dr. Noeggerath, who found some catarrh, a slight erosion, and latero-version; the uterine canal measured one inch longer than normal. This condition of affairs would be called imperfect involution, a state which Dr. Noeggerath believes never to exist unless it is connected with some trouble of the lining membrane of the uterus. Accordingly he used sponge-tents to dilation, and on introducing the finger he felt something like large granulations at the fundus; by scraping he removed two pieces as large as peas, which proved by the microscope to be old villi and degenerated decidua. Dr. Noeggerath feels certain, from the microscopic appearance of the old villi, which were covered with epithelium, that the masses removed were retained in the uterus during the second gestation, and that they were the remains of that portion of the fœtal envelopes which were retained at the time of the abortion six years ago.

Dr. Noeggerath said a second variety of retention occurs in which the secundines are expelled entire and the fœtus remains. He related the following case: A lady came to him saying that she aborted at the third and a half month, and that the physician who was present said that the fœtal envelopes came away, but there was no fœtus expelled. She, however, had no trouble following the abortion, but came to Dr. Noeggerath to find out whether the fœtus was retained or not. On examination Dr. N. found the uterus retroflexed, and the canal larger than normal. In order to bring about menstruation he ordered cold douches, as the uterus seemed relaxed. About four weeks after she expelled a fœtus, entire and perfect in form.

Dr. CHAMBERLAIN mentioned a case which was related to him by a physician whose wife aborted at the third month. The membranes were discharged, but diligent search failed to discover the fœtus. The patient had continuous hemorrhages and suffered from puerperal phlebitis of the lower extremities. Twelve weeks after the expulsion of the membranes a three months' fœtus was expelled in a state of partial decomposition.

Dr. PEASLEE said he presented a case two years ago to the Pathological Society, where seven months elapsed before the ovum was expelled. The patient flooded at the third month, after which there was no increase in the size of the uterus; the husband of the patient was away during the four months previous to the expulsion of the ovum. Dr. P. had seen eight or ten cases where no fœtus could be found on opening the membranes; most of these cases, however, were not older than the ninth or tenth week. There are cases where the placenta retains its vitality indefinitely. He remembers a case like that related by Dr. Chamberlain, in which the membranes were expelled, the patient having many hemorrhages subsequently, and at last went into a typhoid state. Dr. Peaslee was then asked to see her in consultation. On examination he found his finger would easily enter the os, and by pressing high up he detected a mass which he withdrew and found to be a three months' fœtus, which had been retained in all about three months after the expulsion of the membrane.—*Am. Journ. Obstetrics*, Nov. 1871.

Fracture of Second Lumbar Vertebra, etc.; Trephining; Death.—Dr. E. R. WILLARD, of Wilmington, Ill., records (*Chicago Med. Examiner*, Oct. 1871) the following interesting case of this:—

Oct. 5th, 1869, four men stepped upon the cage at the top of the shaft at a coal mine for the purpose of descending into the pit, a distance of one hundred and ninety-five feet. As the engineer started the machinery for lowering the cage, the rope broke, precipitating the cage and men to the bottom, instantly killing one man, producing compound comminuted fracture of the leg of the second, slightly injuring the spine of the third, while the fourth received a severe blow upon the lumbar vertebræ, causing paralysis of the lower extremities. Pins thrust into the skin at any point below the seat of injury gave not the slightest sensation of pain.

The attending physician, Dr. McMann, of Gardner, thinking the case a capital one to try "Clines's Operation" of relieving the compression of the spinal cord by cutting down upon the injured part, and sawing through the lamina sufficiently to elevate the depressed portion of bone, immediately telegraphed for me to meet him in consultation. Upon arriving I learned the above facts, and after a careful survey of the case, notwithstanding the want of success hitherto attending the operation, I advised its immediate performance. So soon as the necessary arrangements were completed, we made an incision about six inches in length, through the integument over the seat of injury, and separated the muscles upon either side of the spinous processes sufficiently to ascertain the extent of injury. This done, we discovered the second lumbar vertebra fractured through the lamina, and the spinous process driven in upon the cord, and so wedged as to be perfectly immovable with the tooth forceps. Hey's saw was applied, and the lamina divided upon either side, and the process elevated, when the spinal cord was found to be so completely contused and lacerated as to entirely obliterate its functional activity.

The wound was properly dressed, the water drawn as occasion required, and the bowels attended to whenever necessary. The patient survived the operation ten days.

—

Colourless Tincture of Iodine.—The usual method of preparing this is by adding successive portions of aqua ammoniæ to the common tincture of iodine until the colour disappears, *i. e.*, until all the iodine has entered into combination with the ammonia. A writer in the *Chicago Druggists' Price Current* suggests as a much more elegant and accurate method, to dissolve at once the iodide of ammonium in dilute alcohol. This will give, it is said, a tincture free from any excess of ammonia or iodides, and be more economical.

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Spontaneous Generation.—The *New York Medical Journal* for February last contains an interesting article on this subject by Prof. J. C. DALTON, in which the author gives the history of our knowledge on spontaneous generation. Prof. D. concludes that now, as always, the idea of spontaneous generation of living beings is confined to organisms of which we know the least. Exactly where our definite knowledge fails, owing either to the minute size or the imperfect organization of these bodies, there commences the obscurity which hangs around their origin. It is very justly said, in support of their spontaneous generation, that, if this mode of production exist at all, it is precisely in the case of the simplest and most imperfect organisms that we should expect it. We might imagine a bacterium or a monad to originate in this way, but not an eagle or an elephant. On the other hand, it is alleged that the imperfect organization of these minute forms is only apparent, and depends on the imperfection in our means of observation. When our microscopes and other aids to investigation have been still further improved, we shall find, it is said, that the bacterium and the vibrio possess an organization of their own, not less essential and complete in its way than that which we now know belongs to the ciliated infusoria. There is every evidence that at least their regular and normal mode of production is from germs disseminated in the atmosphere; and they themselves, as we have already seen, are embryonic or transitional forms in the development of a distinct vegetable growth. They are consequently to be regarded as an inte-

gral part of the cryptogamic vegetable organizations; and, notwithstanding the apparent simplicity of their structure, they no doubt, like other plants and animals, have their definite place in the organic world.

Meat, its Nourishing Properties, etc.—The number of the *Amer. Journal of Pharmacy* for March contains an interesting article on this subject by H. ENDEMANN, Ph.D. The author, to avoid any omission in the enumeration of the component parts of meat, groups all of them together under the general heading "Products of the Decomposition of Albumen."

"Meat consists of fibrin and albumen (about 25 per cent.) and the rest of its solid constituents (about 2½ per cent. in the average) is composed of the products of decomposition of albumen and of alkaline salts. The albuminous substances, fibrin and albumen, represent the nourishing properties of meat, while the salts, possessing likewise nourishing qualities, are important for the promotion of digestion. About twenty years have elapsed since Liebig made his first investigations on the constituents of meat. It was then also that he advanced his views concerning the nourishing properties of the extract of meat, and we find in the "*Chemische Briefe*," published shortly afterwards, his ideas set forth so clearly that the unprofessional reader may understand and duly appreciate them.

"I feel confident that the value of this extract was and is, even now, overestimated. Liebig himself abandoned the idea that the organic constituents of the extract were the agents of its beneficial effects, and experiments, made some years ago in England, show plainly that the ashes of the extract are capable of producing the same effects as the extract itself. Even now, however, after the explosion of the theories that albuminous substances might be built up again from the products of their decomposition, experiments are constantly made to find organic constituents capable of producing the effects of the extract itself, as is evidenced by the recent discovery of carnine, the physiological effect of which is, according to the experiments, more than doubtful. Liebig states that 'the extract, which is produced by extracting meat by cold water, is the nourishment for the muscle;' but the meat liquor is not only the agent of transmitting the nourishment from the blood to the muscles, it also contains the waste products formed during the action of the muscles. Liebig in preparing his extract, however, excludes the real nourishment by coagulating it, and carefully collects the products of decomposition for the good of humanity.

"But, if the alkalies alone constitute the value of this extract, is there not a waste of most valuable material? The interest of the manufacturer will not be disputed, but why does the intelligent consumer pay dollars for that which he might buy for a few cents?

"The fact is, that the public is as yet in the dark; the published experiments are known in most cases only to scientific men and command attention, while the want of support by illustrious names makes them soon forgotten. For the proper utilization of meat, the albuminous as well as the extractive portion must be preserved, for the former not only resupplies the body with albumen, which had become decomposed by the action of the muscles, but serves also as a combustible, while the extractive portion is necessary for a proper digestion. Let us see how these requirements are fulfilled by the methods in vogue for the utilization and preservation of meat.

"When meat is salted, it is treated with an excess of salts (common salt and saltpetre), which absorb the water, forming a concentrated solution, which contains besides these salts much of the extractive portion of the meat. This solution is removed before using the meat, and the latter is even soaked in fresh water for some time, to remove the excess of salts. It is evident that such meat is very poor in extractive salts, and for this reason very difficult to digest.

"The action of smoke depends upon the carbolic or cresylic acid contained therein. These substances coagulate the albumen and fibrin, and thus prevent decomposition. Smoked meat is, therefore, not so easily digested as raw beef, since not only the gastric juice must remove the carbolic acid before digestion

is possible, but the albumen and fibrin, being already coagulated, will resist more strongly the dissolving action of the juice. The conditions will be even more unfavourable for a proper digestion, if the salting and smoking process have been combined.

"One of the most rational of processes of modern invention is the preservation of meat by inclosing it in air-tight cans. This process would undoubtedly give full satisfaction, if it were not for mechanical difficulties, which cannot as yet be surmounted. If properly carried out, however, it is the best process known, because it furnishes the meat in its pure and unadulterated state, the great agent of decomposition, atmospheric air, being excluded.

"When we come to consider the different agents of decomposition, we find that they are, first the atmospheric air with its myriad germs and spores, and secondly water. No decomposition is possible without the latter, and I propose, therefore, the following method of preservation: The meat, after having been cut in slices, should be dried in a hot air-chamber, at a temperature below 140° Fah. If the apparatus is well constructed, the drying may be completed within three hours, if filtered air be drawn rapidly through the chamber.

"In this operation the meat becomes quite hard, and can easily be ground in a mill. It is then in the condition which is best adapted for use. The fibrin and albumen, not being coagulated, are able to take up water, and the fibres expand into their natural state.

"The powder is of a slight brownish-yellow colour; has a trifling odour of roast meat, and keeps exceedingly well. This proves that the salts contained in the meat are entirely sufficient for its preservation, if the quantity of water keeping them in solution is greatly diminished by evaporation.

"Its use is easily understood. For beef soup—two ounces of the powder are boiled for a few minutes with one pint of water and the other usual ingredients. The soup thus prepared will be stronger than that prepared from half a pound of fresh meat, for a solid piece, even after long boiling, will never permit as thorough extracting as the meat powder.

"For solid roast meat dishes, the addition of one egg to a pound of meat powder, together with the requisite quantity of water, suffices to reunite the separated fibres by means of the coagulating egg-albumen.

"The fact that the albumen and fibrin are not coagulated, makes it a valuable medicine for consumptives, and in all cases of debility where good nourishment is requisite. It is even more easily digested than raw meat, for the reason that, if it is taken with cold or lukewarm water, the process of swelling will take place in the stomach, where, being surrounded by gastric juice, the latter is absorbed.

"This I have tested by actual experiment. Corresponding quantities of raw meat and meat powder were digested in glass flasks, under the influence of equal quantities of diluted muriatic acid and pepsin, at a temperature of about 110° Fah. While the contents of the vessel containing the meat powder, after six hours' treatment, represented a uniform, though not quite clear fluid, the vessel containing the raw beef contained yet pieces of the undigested material. A dog was fed for eight days with a daily ration of five ounces of meat powder, corresponding to about one pound of fresh meat. The average weight of the discharges from the rectum was about one-fourth ounce daily (dried at 200° Fah.), the maximum being 8.5 grms., the minimum 5.2 grms. Microscopical examination did not show even traces of undigested meat fibre. The only part of the meat found undigested were the relics of the sinews. Pieces of wood, cork, paper, and threads of the carpets formed, besides the mucous membranes and constituents of the bile, the solid part of the excrements. The dog, who had formerly been fed on mixed food, grew very lively during this treatment. His weight at the end of the treatment was 12½ pounds."

Fraudulent Diploma Traffic.—The Legislature of Pennsylvania in January last appointed a committee "to investigate the facts concerning the alleged corrupt issuing of medical diplomas by any medical college existing under the laws of this State." This committee, on the 20th of March last, presented a report, from which we make the following extracts:—

"The institutions in regard to which inquiry has been made are the University of Pennsylvania, the Jefferson Medical College, the Philadelphia University of Medicine and Surgery, and the Eclectic College of Medicine, all of which are located in the city of Philadelphia.

"In regard to the two first named institutions, the University of Pennsylvania and the Jefferson Medical College, your committee feel bound to report that a full and careful inquiry has developed no cause for suspicion that either the trustees or faculty or any member thereof have in any way been concerned in the unlawful issue of medical diplomas or the improper issue of what are termed 'Honorary Degrees.' On the contrary, the concurrent and uncontradicted testimony shows conclusively that persons connected with these institutions have, from the commencement of this nefarious traffic in diplomas and degrees, used all proper means both to expose and prevent the same, thus fully justifying the high estimate placed upon these institutions and affording new proof of their just title to public confidence and esteem.

"The other institutions, which at present are known by the names of the Eclectic Medical College and the Philadelphia University of Medicine and Surgery, seem to owe their corporate existence to some very complicated and mysterious legislation." * * *

"An examination of many witnesses has convinced your committee that the Philadelphia University of Medicine and Surgery, under the management of Dr. William Payne [Paine?], and the Eclectic Medical College, under the management of Dr. John Buchanan, have for a long time openly engaged in the sale of diplomas to persons who had not attended even a practical collegiate course, and who in many instances were without any medical or scientific attainments whatever.

"It is in evidence that Dr. Payne made an agreement for the sale of diplomas, for the consideration of $200, conferring the degrees of M.D. and LL.D. to a person of whom he knew nothing except the name, and that in pursuance of this arrangement said diplomas were regularly made out and signed. The person named in this instance is stated to have been an infant but two years old. It was also proved that Dr. Payne entered into an agreement with other parties to furnish diplomas for sale. In many instances there was positive proof that he had issued the diplomas of the Philadelphia University of Medicine and Surgery for a consideration to persons who had never attended any course of instruction, and to others who had only attended a few lectures in the course, and almost invariably without requiring an examination of the person so graduated, or the writing of a thesis.

"In a numer of cases witnesses testify to having received meritorious degrees in medicine, without study, examination, or even payment. An examination of the books of the Philadelphia University of Medicine and Surgery discloses the fact than many *honorary degrees* from that institution were disposed of for money, the entries stating specifically the amounts paid for such degrees, and the names of the persons to whom they were sold.

"The testimony concerning the illicit traffic in diplomas by the Eclectic College of Medicine, under the management of Dr. John Buchanan, developed some most astounding facts connected with the management of this institution. It appears to be notorious that the sale of degrees by this college was carried on openly and systematically. Diplomas from this college, in regular form, and signed by the faculty, have been granted to women who could not even tell the location of the college; and there is abundant testimony of the sale of degrees to persons who never attended any of the lectures of the course, or received any medical instruction whatever. One of the faculty testified before the committee that, during the time he filled an important chair in this college, he visited a distant portion of the State for the purpose of ascertaining who among the practising physicians of that locality were without diplomas, with the intention of selling them degrees for whatever sum could be obtained.

"Your committee feel bound to report that this illicit and disgraceful traffic in diplomas by the two colleges above named has brought the medical profession of the State into disrepute, and has done great injury to the character and standing of the University of Pennsylvania and the Jefferson Medical College.

Several members of the faculty of the institutions testified that they are constantly in receipt of letters from distant parts of the country and from England, inquiring upon what terms and conditions diplomas could be purchased, and a number of these letters were produced before your committee, copies of which will be found among the notes of testimony herewith submitted.

"It seems to have been the design of those engaged in the nefarious business to create the impression that they really represented the University of Pennsylvania at Philadelphia, and hence their endeavours, by frequent legislation, to obtain such names as would be likely to be mistaken for it. It is certain from the testimony adduced that both the Eclectic College of Medicine and the Philadelphia University of Medicine and Surgery sold many scholarships to persons who supposed they were purchasing the scholarships of the University of Pennsylvania, and that in no known instance was the error corrected when the student discovered the mistake.

"In view, therefore, of the clearly established fact that both the Eclectic College of Medicine and the Philadelphia University of Medicine and Surgery have abused the trust confided in them by the Legislature by their several acts of incorporation, by selling their degrees to persons who had not attended the regular course of instruction, or were in any manner entitled to hold diplomas from a chartered college, and that their existence as incorporated medical institutions is productive of great disgrace to the medical profession and surgery of the community, we respectfully recommend the passage of a law repealing all former laws incorporating said institutions."

We have great pleasure in stating that the Legislature, to its great honour, has unanimously passed a bill repealing all former laws incorporating the guilty institutions above named.

UNIVERSITY OF PENNSYLVANIA.

MEDICAL DEPARTMENT.

NINTH STREET ABOVE CHESTNUT, PHILADELPHIA.

Spring Course of Lectures and Clinics for 1872.

THE Session will begin Monday, April 1, and continue until June 29.

LECTURERS.

Prof. D. HAYES AGNEW, M.D. . .	Surgery.
WILLIAM PEPPER, M.D.	Clinical Medicine and Physical Diagnosis.
WILLIAM F. NORRIS, M.D. . . .	Diseases of the Eye and Ear.
JAMES TYSON, M.D.	Microscopy and Urinary Chemistry.
WILLIAM GOODELL, M.D.	Diseases of Women and Children.
H. LENOX HODGE, M.D.	Regional Anatomy.
JAMES E. GARRETSON, M.D. . . .	Surgical Diseases of the Mouth.
GEORGE STRAWBRIDGE, M.D. . . .	Diseases of the Eye and Ear.
LOUIS A. DUHRING, M.D.	Diseases of the Skin.

The FEE for the Course upon Regional Anatomy is $10.
All the other Lectures and Clinics are free to Matriculates.

R. E. ROGERS, M.D., *Dean.*

TO MEDICAL GRADUATES.

DR. HORATIO ROBINSON STORER (late Professor at the Berkshire Medical College) will deliver his Ninth Private Course of Twelve Lectures on the Treatment of the Surgical Diseases of Women, during the first fortnight of June, 1872, commencing on Saturday, June 1, at 4 P.M. Fee, $50.

The Course is intended to cover all that is yet known of Pelvic Surgery in the Female; and while interesting both to general and special practitioners, it commends itself particularly to those teaching, or desiring to teach, gynæcology at hospitals and medical colleges.

Applicants for the certificate of attendance will be required, as in previous years, to furnish evidence of good professional standing, as defined by the American Medical Association.

BOSTON, March 15, 1872.

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